

# TRACTS:

CONTAINING

I. SUSPICIONS about some *Hidden Qualities* of the *AIR*; with an *Appendix* touching *CELESTIAL MAGNETS*, and some other Particulars.

II. ANIMADVERSIONS upon Mr.  *Hobbes's PROBLEMATA De VACUO.*

III. A DISCOURSE of the CAUSE of ~~SUCTION~~ by Attraction by *SUCTION.*

By the Honourable  
**ROBERT BOYLE, Esq;**  
Fellow of the ROYAL SOCIETY.

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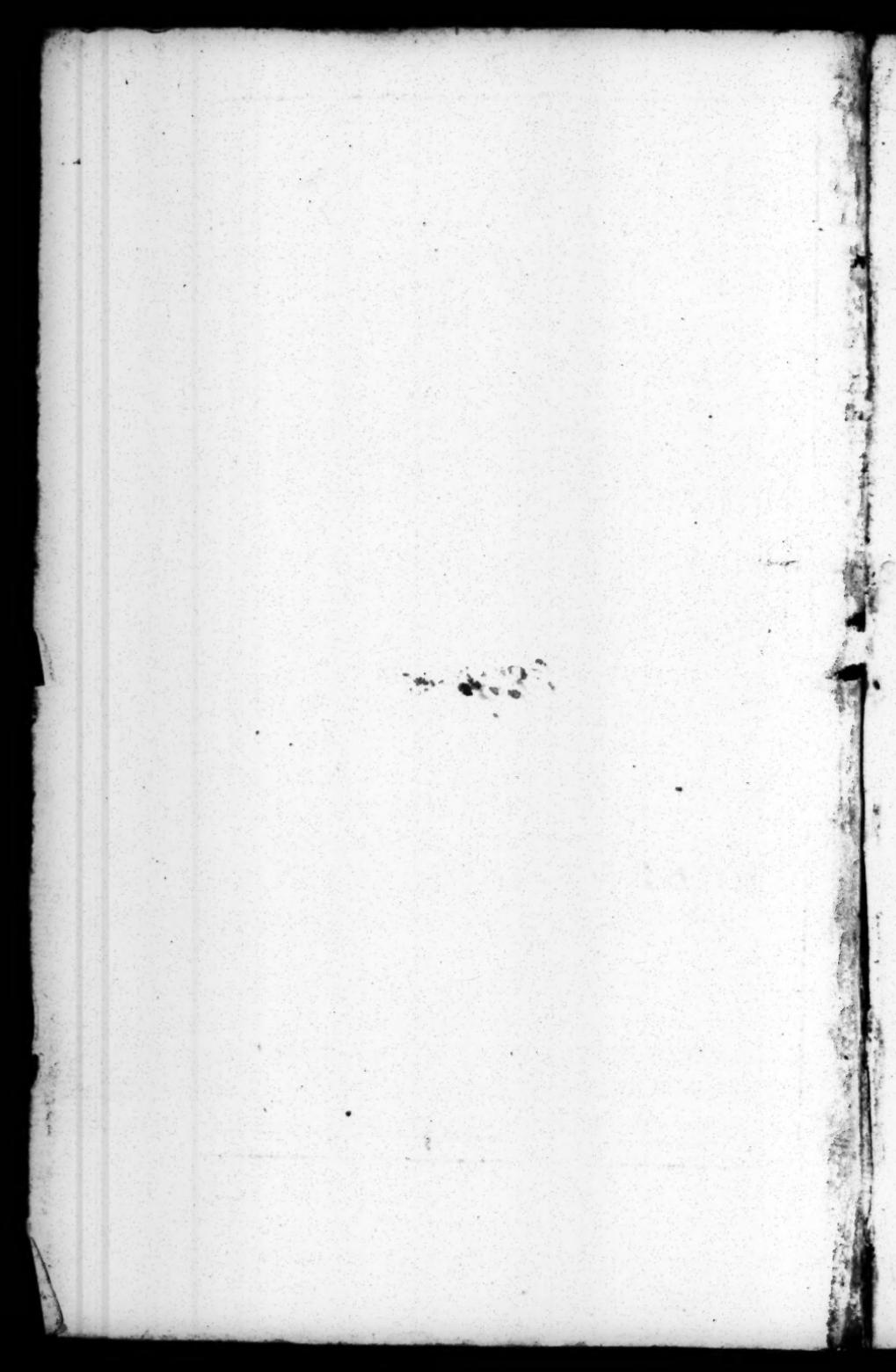
L O N D O N,

Printed by *W.G.* and are to be Sold by *M.Pitt,*  
at the *Angel* against the Little North Door  
of St. Paul's Church. 1674

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# Preface.

**A**mong other Papers that I design'd to contribute towards the Natural History of the Air, I began some years ago to set down a Collection of some new or less heeded Observations and Experiments relating to the Causes and Effects of Changes in the Air, which I refer'd to several Heads, as to the Airs Heat, Coldness, Moisture, Driueness, Diaphaneity, Opacity, Consistence, several Saltnesses and other Titles; the last of which was

A 2                    of

## P R E F A C E.

of the Occult Qualities of the Air,  
supposing there be any such. And  
though afterwards I was, by Sick-  
ness and other Impediments, di-  
verted from proceeding in that  
Collection, and induc'd to lay aside  
some of the Observations I had  
provided, and employ in other  
Treatises such as were proper to  
them; yet as to the Title that con-  
tain'd Suspicions about some  
Hidden Qualities of the Air,  
the possibility, if not likelihood,  
that either the Matters of fact, or  
the Intimations delivered in them,  
might afford hints not useless to  
the Sagacious and Inquisitive, per-  
suaded me to let it escape the Fate  
of its Companions, though possi-  
bly, if I had more consulted my  
own Reputation, I should least of  
all have suffer'd this Title to ap-  
pear,

## P R E F A C E.

pear, there being none of the rest,  
that was not less conjectural. But  
it being thought unfit, that any  
thing should perish, that related to  
so considerable and uncommon a  
Subject, as that of this Title, I  
was content to cast the collected  
Experiments into the following  
Essay, for the Reasons express'd at  
the beginning and close of the in-  
suing Paper. Which, 'twas hop'd,  
may be the better understood, and  
less liable to have its Design mi-  
staken, by being usher'd in by this  
Advertisement about the occasion  
of it.

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## ERRATA.

**I**N the first Tract, pag. 41. l. 4. read  
Halicarnasseus.

*In the Tract of the Cause of Suction;*  
p. 14. l. 4. r.  $33\frac{1}{2}$  for  $36\frac{1}{2}$ .

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SUSPICIONS  
ABOUT  
Some *Hidden Qualities*  
in the *AIR.*

**B**esides the four first Qualities of the Air, (Heat, Cold, Dryness and Moisture) that are known even to the Vulgar; and those more unobvious, that Philosophers and Chymists have discovered, such as Gravity, Springiness, the power of Refracting the beams of Light, &c. I have often suspected, that there may be in the Air some yet more latent Qualities or Powers differing enough from all these, and principally due to the Substantial Parts or Ingredients, whereof it consists:

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sists. And to this conjecture I have been led, partly (though not only or perhaps chiefly) by considering the Constitution of that Air we live and breath in, which, to avoid ambiguities, I elsewhere call *Atmospherical Air*. For this is not, as many imagine, a Simple and Elementary Body, but a confus'd Aggregate of Effluviums from such differing Bodies, that, though they all agree in constituting, by their minutenes and various motions, one great mass of Fluid matter, yet perhaps there is scarce a more heterogeneous Body in the world.

And as by Air I understand not (as the *Peripateticks* are wont to do) a meer Elementary Body; so, when I speak of the Qualities of the Air, I would not be thought to mean such naked and abstracted Beings (as the *Schools* often tell us of,) but such as they call Qualities *in concreto*, namely Corpuscles indued with Qualities, or capable of producing them in the Subjects they invade and abound in.

I have elsewhere shewn it to be highly probable, that, besides those vapours and exhalations which by the

*In a Paper a-  
bout Subter-  
raneal Steams.*

Heat of the Sun are elevated into the Air, and there afford matter to some Meteors, as Clouds, Rain, Parheliions and Rainbows, there are, at least at some times, and in some places, store of Effluviums emitted from the Subterraneal parts of the Terrestrial Globe; and 'tis no less probable, (from what I have there and elsewhere deliver'd,) that in the Subterraneal Regions there are many Bodies, some fluid and some consistent, which, though of an operative nature, and like upon occasion to emit steams, seldom or never appear upon the surface of the Earth, so that many of them have not so much as names assigned them even by the Mineralists. Now among this multitude and variety of Bodies, that lye buried out of our sight, who can tell but that there may be some, if not many, of a nature very differing from those we are hi-

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thereto familiarly acquainted with ;  
and that , *as* divers wonderful and  
peculiar operations of the *Loadstone*,  
(though a Mineral many Ages ago fa-  
mous among Philosophers and Phy-  
sicians,) were not discovered 'till of  
later Ages, wherein its nobler Virtues  
have been disclosed ; *so* there may be  
other Subterraneous Bodies , that are  
indowed with considerable powers ,  
which to us are yet unknown , and  
would, if they were known, be found  
very differing from those of the *Fossiles*  
we are wont to deal with :

I also further consider , that , (as I  
have elsewhere endeavoured to make  
it probable) the Sun and Planets (to  
say nothing of the Fixt Stars) may  
have influences here below distinct  
from their Heat and Light. On which  
Supposition it seems not absurd to me  
to suspect , that the Subtil , but Cor-  
poreal , Emanations even of these Bo-  
dies may (sometimes at leaft) reach  
to our Air , and mingle with those  
of our Globe in that great receptacle  
or rendevouz of Celestial and Ter-  
restrial

## **Hidden Qualities in the Air.** 5

restrial Effluviums, the *Atmosphere*. And if this suspition be not groundless, the very small knowledge we have of the structure and constitution of Globes so many thousands or hundred of thousands of miles remote from us, and the great ignorance we must be in of the nature of the particular Bodies that may be presum'd to be contain'd in those Globes, (as Minerals and other Bodies are in the Earth,) which in many things appear of kin to those that we inhabit, (as with excellent Telescopes I have often with attention and pleasure observed, particularly in the Moon,) this great imperfection, I say, of our knowledge may keep it from being unreasonable to imagine, that some, if not many, of those Bodies and their effluxions may be of a nature quite differing from those we take notice of here about us, and consequently may operate after a very differing and peculiar manner.

And though the chief of the Heteroclite Effluviums, that inow the

Air with hidden Qualities , may probably proceed from beneath the surface of the Earth , and from the Celestial Bodies ; yet I would not deny but that , especially at some times and in some places , the Air may derive multitudes of efficacious particles from its own operations , acting as a fluid Substance upon that vast number and variety of Bodies that are immediately expos'd to it . For , though by reason of its great thinness , and of its being in its usual state devoid both of taſt and ſmell , it ſeems wholly unfit to be a *Menſtruum* ; yet I am not ſure but it may have a diſſolving , or at leaſt a conſumming , power on many Bodies , eſpecially ſuch as are peculiarily dispos'd to admit its operations .

For I conſider , that the Air has a great advantage by the vaſt Quantity of it , that may come to work in proportion to the Bodies that are expos'd to it : And I have long thought , that , in divers caſes , the Quantity of a *Menſtruum* may much more conſiderably

rably compensate its want of strength, than Chymists are commonly aware of, (as there may be occasion elsewhere to exemplifie.) And there are liquors, which pass for insipid, (and are therefore thought to be altogether unfit to be Solvents,) which, though they have their active parts too thinly dispersed to be able presently to make sensible Impressions upon our Organs of Tasting, yet are not quite destitute of Corpuscles fit to act as a Solvent; especially if they have time enough to make with the other parts of the Fluid such numerous and various motions, as must bring, now some of them, and then others, to hit against the Body expos'd to them. Which may be illustrated by the Rust like to Verdigrase, which we have observ'd in Copper that has been long expos'd to the Air, whose saline particles, little by little, do in tract of time fasten themselves in such numbers to the surface of the Metal as to corrode it, and produce that efflorescence colour'd like Verdigrase

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grease, which you know is a factious Body, wont to be made of the same Metal, corroded by the sharp Corpuscles of Vineger, or of the Husks of Grapes: Besides, that by the power, which *Mercury* has to dissolve Gold and Silver, it appears, that it is not always necessary for the making a Fluid fit to be a Dissolvent, that it should affect the Tast. And as to those Bodies, on which the *Aerial Menstruum* can, though but slowly, work, the greatest quantity of it may bring it this advantage, that, whereas even the strongest *Menstruum*, if they bear no great proportion in bulk to the Bodies they are to work on, are easily glutted, and being unable to take up any more, are fain to leave the rest of the Body undissolved, our *Aerial Menstruum* bears so vast a proportion to the Bodies expos'd to it, that when one portion of it has impregnated it self as much as 'tis able, there may still come fresh and fresh to work further on the remaining part of the expos'd Body.

Besides

## Hidden Qualities in the Air. 9

Besides the Saline and Sulphureous particles, that, at least in some places, may (as I have elsewhere shewn) impregnate the Air, and give it a greater affinity to Chymical *Menstruum*s more strictly so called; I am not averse from thinking, that the Air, meerly as a *fluid Body*, that consists of Corpuscles of differing sizes and solidities restleſſly and very variously moved, may upon the account of these Corpuscles be still resolving, or preying upon, the particles of the Bodies that are expos'd to their action. For, many of those Aerial Corpuscles, some hitting and some rubbing themselves every minute against those particles of expos'd Bodies that chance to lye in their way, may well, by those numerous occursions and affrietions, strike off and carry along with them now ſome and then others of those particles; as you ſee it happens in water, which, as soft and fluid as it is, wears out ſuch hard and ſolid Bodies as Stones themſelves, if it often enough meet them in its paſſage,

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passage, according to the known  
saying,

*Gutta cavat lapidem non vi, sed sape  
cadendo.*

And though the Aerial Corpuscles  
be very minute, and the Bodies expos'd  
to them oftentimes large and seem-  
ingly solid; yet this needs not make  
you reject our supposition, because  
'tis not upon the whole Body at once,  
that, according to us, the Aerial  
Corpuscles endeavour to work, but  
upon the Superficial particles, which  
may often be more minute than those  
Corpuscles; as you will the more ea-  
sily believe, if you *first* observe with  
a good Microscope, how many ex-  
tant particles may be met with on the  
surface of Bodies, that to the naked  
Eye seem very smooth, and even of  
those that are polish'd by Art with  
Tripoly or Puttee; and *then* consider,  
that one of these protuberancies, be-  
ing yet manifestly visible, may well  
be presum'd to consist of a multitude  
of

of lesser particles, divers of which may very well be as minute as those Aerial Corpuscles, that successively hit against them, and endeavour to carry them along with themselves. And this may be illustrated by a familiar instance. For, if you take a lump of *Loaf Sugar*, or even of a much softer and harder Body, *Sal Gemmae*, and cast it into common water, though this liquor be insipid, and the motions of its corpuscles but very languid; yet these corpuscles are capable to loosen and carry off the superficial particles of Sugar or Salt, that chance to lye in their way, and fresh corpuscles of water still succeeding to work upon the remaining particles of the expos'd Body that stand in their way, the whole lump is by little and little dissolved, and ceases to appear to the Eye a thing distinct from the liquor.

Some things that have occur'd to me have made me suspect, that 'tis not impossible, but that some Bodies may receive a disposition to *Volatility*, and

and consequently to pass into the Air by the action either of the Sun-beams, in the form of the Sun-beams, or of some substance that once issued out of the Sun and reach'd unto our Air. For, there may be certain Bodies for the most part in the form of liquors, which, though they pass off from some peculiarly dispos'd Bodies, may during their stay or contact produce in them a great and strange aptness to be volatiliz'd. In favour of which conjecture, I might here alledge *both* the effects, which the *Paracelsians* and *Helmontians* ascribe to the *Alkahest* of volatilizing even fixt and ponderous Bodies barely by being often abstracted from them, *and* some other things, which I shall now leave unmention'd, because you may find them in my Notes about *Volatility and Fixity*.

But, whatever become of this *Conjecture*, 'tis consonant to *Experience*, that, either upon the above-recited accounts, or also some others, those parts of the Atmosphere, which, in  
a stri-

a stricter sense, may be call'd the *Air*, are, at least in some places, so intermixt with particles of differing kinds, that among that great number of various sorts of them, 'tis very likely that there should be some kinds of an un-common and an unobserved nature. And I could countenance what has been said by the wasting of *Odorous Bodies*, and especially *Camphire*, and by representing, that I have observed some solid Bodies actually cold, when their superficial parts were newly taken off, to emit, though invisibly, such copious steams into the *Air*, as to grow continually and manifestly lighter upon the balance, so as to suffer a notable decrement of weight in a minute of an hour. But the mention I make of such things in an other paper, dissuades me from insisting on them here, where 'twill be seasonable to resume the discourse, which the mention of the *Dissolving power*, that may be guess'd to be in the *Air*, has for some pages interrupted, and to tell you,

you, that those propounded, before I enter'd upon the digression, are the two main Considerations *à priori* (as they speak) whereon I have ground'd my surmize, which being propos'd but as a *Suspicion*, I presume it will not be expected, that the Arguments *à posteriori*, which I shall bring to countenance it, should be more than *Conjectures*, much les that they should be *Demonstrations*. And therefore I shall venture to lay before you some few *Phænomena*, which seem to be at least as probably referable to some *latent Quality in the Air*, as to any other cause I yet know. Upon which score such *Phænomena* may be allowed to be pleaded in favour of our Suspicion, 'till some other *certain* cause of them shall be satisfactorily assign'd.

Having premis'd thus much to keep you from looking for stronger proofs than I think my task obliges me to give; the *first Phænomenon*, I shall propose, shall be the appearing or growth of some *Salts* in certain Bodies,

Bodies, which we observ'd to afford them either not at all, or at least nothing near in such plenty, or so soon, unless they be expos'd to the Air. Of such a Phænomenon as this, that is not so much as mention'd by Vulgar Philosophers, and very rarely, if at all, to be met with in the Laboratories of Chymists, you will not, I suppose, wonder, that I do not present you many Examples, and some few I am able to name. For I remember, that suspecting a solid Marchasite, hard as stone, to be fit to be made an instance for my purpose, I caus'd it to be broken, that the internal more shining parts might be expos'd to the Air; but, though this were done in a room, where a good fire was usually kept, so that the Marchasite was not only shelter'd from the rain, but kept in a dry Air, yet after a while I discover'd upon the glistering parts an efflorescence of a vitriolate nature.

And afterwards meeting with a ponderous and dark colour'd Mineral,  
and

and which , at the first breaking ,  
discover'd to the Eye no appearance  
of any Salt , nor so much as any shi-  
ning Marchasitical particles , we found  
nevertheleſs , that a good quantity of  
these hard and heavy Bodies , being  
kept expos'd to the Air ; even in a  
room that preserved them from the  
rain , though probably they had lain  
many ages intire in the hill , wherein  
they were found under ground ; yet  
in not many months , by the opera-  
tion of the Air upon them , they were ,  
in great part , crumbled to powder  
exceeding rich in Copperas . Nay ,  
I remember , that having for Curio-  
ſities fake , laid up ſome of these  
ſtones in a room , where I conſtantly  
kept fire , and in the drawer of a Ca-  
binet , which I did not often take out  
to give them fresh Air , ſome , if not  
moſt of them , were notwithstanding  
cover'd with a copious efflorefcence ,  
which by its conſpicuous colour be-  
tween blew and green , by its taste ,  
and by its fitneſs to make in a trice  
an inky mixture with infusion of  
galls ,

galls, sufficiently manifested it self to be *Vitriol*; whose growth by the help of the contact of the Air is the more considerable, because it is not a meer Acid Salt, but abounds in Sulphureous and Combustible parts, which I have divers times been able; by Methods elsewhere mentioned, actually to separate or obtain from common Vitriol without the addition of any combustible body, and sometimes without any additament at all. It was also uncommon, that our blackish Minerals requir'd no longer time, nor no rain, to make them afford their *Vitriolate Efflorescences*: For I remember, I kept many of those Marchasites, both glittering ones and others, of which they make and sell great quantities of Vitriol at *Deptford*, without perceiving in them a change that came any thing near to what I have recited. And I observ'd those, whose trade it is to make Vitriol, to be often obliged to let their Vitriol-stones, as they call them, lye half a year, or even eighteen months; or

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two years expos'd, not only to the open Air, but to the Rain and Sun, to be able to obtain from them their Vitriolate parts.

That also the Earth or Ore of Alum, being robb'd of its Salt, will in tract of time recover it by being expos'd to the Air, we are assur'd by the experienc'd *Agricola*, where, having deliver'd the way of making *Allum*, he subjoins this Advertisement: *Terra Aluminosa, quæ in castellis diluta, postquam effluxit, superfuit egesta et coacervata quotidie, rursus magis & magis fit aluminosa, non aliter atque terra ex qua halinitrum fuit confectum, suo succo plenior fit; quare denuo in Castella conjicitur & aquæ affusæ ea percolantur.*

I have likewise observ'd, as you also perchance have done, that some kind of *Lime* in old walls and moist places has gain'd in length of time a copious efflorescence, very much of a *Nitrous* Nature; as I was convinc'd by having obtain'd *Salt-peter* from it by barely dissolving it in common water,

water, and evaporating the filtrated Solution: And that in calcin'd Vitriol, whose saline parts have been driven away by the violence of the fire, particles of fresh Salt may be found after it has lain a competent time in the Air, I shall e're long have occasion to inform you.

But in the mean time, (to deal ingenuously with you,) I shall freely confess to you, that, though these and the like observations have satisfied Learned men, without having been call'd in question, and consequently have, at least, probability enough to ground our Suspicion upon; yet I, that am more concern'd for the *Discovery* of a *Truth* than the *Reputation* of a *Paradox*, propose the Argument drawn from the foregoing Observations, but as a Probationer. For it yet seems to me somewhat doubtful, whether the Salts, that appear in the forementioned cases, are really produc'd by the operation of the Air working as an Agent, or also concurring as an Ingredient; or whe-

ther these saline substances be not the production of some internal thing that is analagous to a Seminal Principle, which makes in these bodies a kind of maturation of some parts, which being once ripen'd, and perhaps assisted by the moisture of the Air, disclose themselves in the form of saline Concretions ; as in the ferculent or Tartareous parts of many Wines there will in tract of time be generated or produc'd store of Corpuscles of a saline nature, that produce the acid taste we find in *Tartar*, especially that of *Rhenish* wine. It may also be suspected, that the formerly mention'd Salts found in Mar-chasites, in Nitrous and Aluminous Earths, &c. are made by the saline particles of the like nature, that among multitudes of other kinds swim in the Air, and are attracted by the congenerous particles, that yet remain in the Terrestrial bodies, that are, as it were, the wombs of such Minerals, (as I have elsewhere shewn, that Spirit of Nitre will, with fixt  
Nitre

Nitre and some other Alkalys, compose Salt-peter;) or else, that these Aerial Salts, if I may so call them, assisted by the moisture of the Air, do soften and open, and almost corrode or dissolve the more Terrestrial Substance of these wombs, and thereby sollicit out and somewhat extricate the latent Saline particles, and, by their union with them, compose those Emerging bodies that resemble *Vitriol, Allum, &c.*

But not only to suggest these scruples, as if I had a mind they should but trouble you, and keep you irresolute, I shall propound something towards the removal of them; namely, that a convenient quantity of Nitrous Earth, or that other of those Substances, which you would examine, be kept in a close vessel to which the Air has not access, for at least as long time as has been observed to be sufficient to impregnate the like substance, or rather a portion of the same parcel that was chosen to be included: For, if the body, that

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was kept close, have either gain'd no Salt at all, or very much less in proportion to its bulk than that which was kept expos'd, we may thence estimate, what is to be ascribed to the Air in the production of Nitre or other saline Concretions. And, because I have observed none of these bodies, that would so soon, and so manifestly, even to the eye, disclose a saline substance, as the blackish Vitriol-Ore, I lately told you I kept in a drawer of my Cabinet; I judg'd *that* a very fit subject, wherewith to try, what maturation or time, when the Air was secluded, would perform towards the deciding of our Difficulty: And accordingly having taken some fragments of it, which we had carefully freed from the adhering Vitriolate efflorescence, by whole plenty we were assured that it was very well dispos'd to be wrought on by the Air, we put of these fragments of differing sizes into two conveniently shap'd glasses, which being Hermetically sealed were ordered

to

to be carried away, and kept in fit places; by which means 'twas expected, that, even without opening the glasses, we should be able easily to see by the chang'd colour of the superficial parts, whether any Vitriolate efflorescence were produced; but, through the negligence or mistake of those, to whom the care was recommended, the experiment was never brought to an issue; and though I afterwards got more of the Mineral, and made a second tryal of the same, I have not yet been inform'd of the event.

But, *Sir*, though, 'till the success of some such tryal be known, I dare not too confidently pronounce about the Production or Regeneration of Salts in bodies that have been robb'd of them, and ascribe it wholly to the Air; yet, when I consider the severall and great effects of the Air upon divers other bodies, I think it not rash to conjecture in the mean time, that the operations of the Air may have a considerable share in these

*Phænomena*, and so that there may be *latent Qualities* in the Air, in the sense I declar'd above, where I told you, that, when I speak of these Qualities, I look upon them in *Concreto*, (as they phrase it,) together with the Substances or *Corporeal effluvia* they reside in: And of these Aer-  
ial Qualities, taken in this sense, I shall now proceed to mention some other Instances.

The Difficulty we find of keeping Flame and Fire alive, though but for a little time, without Air, makes me sometimes prone to suspect, that there may be dispers'd through the rest of the Atmosphere some odd substance, either of a Solar, or Astral, or some other exotic, nature, on whose account the Air is so necessary to the subsistence of Flame; which Necessity I have found to be greater, and less dependent upon the *manifest Attributes* of the Air, than Naturalists seem to have observed. For I have found by tryals purposely made, that a small flame of a Lamp, though fed

fed perhaps with a subtil thin Oyl, would in a large capacious glass-Receiver expire, for want of Air, in a far less time than one would believe. And it will not much lessen the difficulty to alledge, that either the gross fuliginous Smoak did in a close Vessel stifle the flame, or that the pressure of the Air is requisite to impel up the aliment into the wieck: For, to obviate these objections, I have in a large Receiver employ'd a very small wieck with such rectified Spirit of Wine, as would in the free Air burn totally away; and yet, when a very small Lamp, furnished (as I was saying) with a very slender wieck, was made to burn, and, fill'd with this liquor, was put lighted into a large Receiver, that little flame, though it emitted no visible smoak at all, would usually expire within about one minute of an hour, and, not seldom, in a less time; and this, though the wieck was not so much as sing'd by the flame: Nor indeed is a wieck necessary for the experiment,

ment, since highly rectified Spirit of Wine will in the free Air flame away well without it. And indeed it seems to deserve our wonder, what that should be in the Air, which inabling it to keep flame alive, does yet, by being consum'd or deprav'd, so suddenly render the Air unfit to make flame subsist. And it seems by the sudden wasting or spoiling of this fine Subject, whatever it be, that the bulk of it is but very small in proportion to the Air it impregnates with its virtue. For after the extinction of the flame, the Air in the Receiver was not visibly alter'd, and, for ought I could perceive by the ways of judging I had then at hand, the Air retain'd either All, or at least far the greatest part of its *Elasticity*, which I take to be its most genuine and distinguishing property.

And this *undestroy'd springyness* of the Air seems to make the necessity of fresh Air to the Life of *hot Animals*, (few of which, as far as I can guess after many tryals, would be

be able to live two minutes of an hour, if they were totally and all at once deprived of Air,) suggest a great suspicion of some *vital substance*, if I may so call it, diffus'd through the Air, whether it be a *volatile Nitre*, or (rather) some *yet anonymous substance*, Sydereal or Subterraneal, but not improbably of kin to that, which I lately noted to be so necessary to the maintenance of other flames.

I know not, whether you will think it pertinent to our present Discourse, that I observe to you, that by keeping putrifying bodies in glasses, which by *Hermes* his seal were secur'd from the contact of the external Air, I have not been able to produce any Insect or other living Creature, though sometimes I have kept *Animal Substances* and even Blood so included for many months, and one or two of them for a longer time; and though also these Substances had a manifest change made in their consistence whilst they remain'd seal'd up.

On

On this occasion I shall add an odd Observation, that I met with in a little Dissertation *de admirandis Hungarie aquis*, written by an Anonymous, but Ingenious, Nobleman of that Countrey, where, speaking of the native Salt that abounds in their Regions, he says, that in the chief Mine (by them call'd *Desiensis*) of *Transsylvania*, there was, a few years before he writ, a great Oak like a huge beam dug out of the middle of the Salt; but, though it was so hard, that it would not easily be wrought upon by Iron-tools, yet being expos'd to the Air out of the Mine, it became so rotten, as he expresses it, that in four days it was easie to be broken and crumbled between ones fingers. And of that corruptive or dissolutive Power of the Air near those Mines, the same Author mentions other Instances.

Having found an Antimonial Preparation to procure Vomits, in a case where I did not at all expect it, I was afterwards curious to inquire of

of some Physitians and Chymists, that were of my Acquaintance, whether they had not taken notice, that *Antimonium Diaphoreticum*, which, as its name imports, is wont to work by sweat or transpiration, would not become vomitive, if 'twere not kept from the Air? To which one Physitian, that was a Learned Man, as sur'd me, it would, as he had found by particular tryals: And the like answer has been given me by more than one. And I find, that the experienc'd Zwelfer himself does somewhere give a caution against letting the Air have access to these Antimonial Medicines, lest it should render them, as he says it will, in tract of time, not only Emetic, but dispos'd to produce heart-burnings, (as they call them,) faintings, and other bad Symptoms. And I learnt by inquiry from a very Ingenious Doctor of Phylic, that, having carefully prepar'd *Antimonium Diaphoreticum*, he gave many doses of it whilst it was fresh and kept stopt in a glass (without finding

finding that in any Patient it procur'd so much as one vomit,) but having kept a parcel of the self same Remedy for a pretty while in a glas only cover'd loosely with a paper, the Medicine, vitiated by the Air, proved *emetic* (strongly enough) to those, who neither by Constitution, or foulness of Stomach, or on any other discernable account, were more than others that had taken it dispos'd to vomit. By which Observations, and from what I formerly told you of the Salt-peter obtainable from Quick-lime, a Man partial to the Air would be made forward to tell you, that this looks, as if either there were in the Air a substance dispos'd to be assimilated by all kinds of bodies, or that the Air is so vast and rich a Rendezvous of innumerable *seminal* Corpuscles and other Analogous particles, that almost any body long expos'd to it may there meet with particles of kin to it, and fit to repair its wrongs and losses, and restore it to its natural Condition. But without

out taking any further notice of this odd surmize, I will proceed to mention two or three other *Phænomena* of Nature, that seem to favour the Suspicion, that there may be *Secret Qualities* in the Air in reference to some bodies.

The ingenious *Monsieur de Rochefort*, in the handsom account he gives of the Apple or Fruit of the Tree *Junipa*, whose juice is employ'd by the *Indians* to black their skins, that they may look the more terrible to their Enemies, observes, that, though the stain, or, as he speaks, the Tincture of this Fruit cannot be wash'd out with Soap, yet within nine or ten days it will vanish of it self; which would make one suspect, that there may be in the Air some secret powerful substance, that makes it a *Menstruum* of more efficacy than Soap it self to obliterate stains. I remember, I have seen this Fruit, but not whilst it was succulent enough to have a tryal made with it; which I was therefore troubled at, because  
the

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the Author does not clearly express, whether this disappearing of the tincture happens indifferently to the bodies it chances to stain, or only is observed on the skins of *Men*. For, as in the former Case 'twill afford an instance pertinent to our present purpose; so in the latter I should suspect, that the vanishing of the tincture may be due not so much to the operation of the Air upon it, as to the sweat and exhalations of a human body, which abounding with volatile Salt, may either destroy or carry off with them the colour'd particles they meet with in their passage.

I have sometimes, not altogether without wonder, observ'd the excellency of the better sort of *Damasco-steel*, (for I speak not of all that goes under that name,) in comparison of ordinary steel. And, besides what I have elsewhere taken notice of concerning it, there is one *Phænomenon*, which though I am not sure it belongs to the latent Qualities of the Air, yet because it may well do so, and I

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am unwilling it should be lost, I will here tell you, that having inquired of an eminent and experienc'd Artificer, (whom I long since employed in some difficult Experiments,) about the properties of *Damasco-steel*, this honest and sober Man averr'd to me, that when he made Instruments of it, and gave them the true temper, which is somewhat differing from that of other Steel, he generally observed, that though, when Razors or other Instruments made of it were newly forged, they would be sometimes no whit better, and sometimes less good, than those made of other Steel; yet when they had been kept a year or two or three in the Air, though nothing else were done to improve them, they would be found much to surpas other Instruments of the same kind, and what themselves were before; in so much, that some of them have been laid aside at first, as no way answering the great expectation conceived of them, which after two or three years were found

to surpass it ; of which also I am now making a tryal. I have several times made a substance that consists chiefly of a Metalline body , and is of a texture close enough to lye for many hours undissolv'd in a Corrosive Menstruum ; and yet this substance, that was fixt enough to endure the being melted by the Fire without losing its colour , would , when I had purposely expos'd it to the Air , be discoloured in a very short time , and have its superficial parts turned almost black .

And this brings into my mind that very pretty Observation , that has been newly made in *Italy* by an ingenious Man , who took notice , that , if after the opening of a Vein the blood be kept 'till it be concreted , and have excluded the superficial *serum* , though the lower part be usually of a dark and blackish colour in comparison of the superficial parts , and therefore be counted far more feculent ; yet , if the lump or clott of blood be broken , and the internal  
and

and dark coloured parts of the blood be expos'd to the Air, it will after a time (for 'tis not said how long) be so wrought on by the contact of the Air, that the superficial part of the blood will appear as florid as the lately mention'd upper part (suppos'd to be, as it were, the flower of the blood,) did seem before. And this Observation I found to hold in the blood of some *Beasts*, whereon I tryed it, in which I found it to succeed in much fewer minutes, than the *Italian Virtuoso's* Experiment on *Human* blood would make me expect.

On the other side I have often prepar'd a Substance, whose effect appears quite contrary to this. For, though this factitious Concrete, whilst kept to the Fire or very carefully preserved from the Air, be of a red colour almost like the common opacous *Bloodstone* of the shops; yet, if I broke it, and left the lumps or fragments of it a little while in the Air, it would in a short time (sometimes perhaps not amounting to a

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quarter of an hour) it would, I say, have its superficial parts turn'd of a very dark colour, very little, and sometimes scarce at all, short of blackness.

A very inquisitive Person of my acquaintance, having occasion to make, by Distillation, a Medicine of his own devising, chanc'd to observe this odd property in it, That at that time of the year, if it were kept stopt, it would be coagulated almost like Oyl of Anniseeds in cold weather; yet, if the stopple were taken out, and so access were for a while given to the Air, it would turn to a liquor, and the vessel being again stopt, it would, though more slowly, recoagulate. The hints, that I guess'd might be given by such a Phænomenon, making me desirous to know something of it more than barely by Relation, I express'd rather a curiosity than a diffidence about it; and the Maker of it telling me, he thought, he had in a small Vial about a spoonful of this Medicine left in a  
neigh-

neighbouring Chamber, I desired his leave to consider it my self, which Request being presently complied with, I found it, when he brought it into the Room which I stayed in, not liquid but consistent, though of but a flight and soft contexture. And having taken out the Cork, and set the Vial in a window, which (if I well remember) was open, though the Season, which was Winter, was cold, yet in a little time that I stayed talking with the Chymist, I found, that the so lately coagulated substance was almost all become fluid. And another time, when the Season was less cold, having occasion to be where the Vial was kept well stopt, and casting my Eyes on it, I perceiv'd the included substance to be coagulated much like Oyl of Anniseeds. And this substance having, as the Maker assur'd me, nothing at all of Mineral in it, nor any Chymical Salt, it consisting only of two simple bodies, the one of a vegetable and the other of an animal substance, distill'd together,

ther, I scarce doubt but you will think with me, that these contrary operations of the Air, which seems to have a power in some Circumstances to coagulate such a body, and yet to dissolve and make it fluid when fresh and fresh parts are allow'd access to it, may deserve to be further reflected on, in reference (among other things) to the opportune operations, the inspired Air may have on the consistence and motion of the circulating blood, and to the discharge of the fuliginous recrementa to be separated from the blood in its passage through the Lungs.

There are two other *Phænomena* that seem favourable to our Suspicion, *That there are Anonymous Substances and Qualities in the Air*, which ought not to be altogether prætermitted on this occasion; though, because to speak fully of them would require far more time than I can now spare, I shall speak of them but succinctly.

The latter of these two *Phænomena* is

is the growth or appearing production of *Metals* or Minerals dug out of the Earth, and expos'd to the Air. And this, though it be the last of the two, I mention first, because it seems expedient, lest it should prove too long an interruption to our Discourse, to postpone the Observations and annex them to the end of this Paper; only intimating to you now, that the caution I formerly interposed about the Regeneration of *Salts* in Nitrous and other Earths, may, for greater security, be applied, *mutatis mutandis*, to that production of Metalline and Mineral bodies we are speaking of.

The other of the two *Phænomena*, <sup>nature of Epidem.</sup> I lately promis'd to mention, is afforded me by those various and odd Diseases, that at some times and in some places happen to invade and destroy numbers of *Beasts*, sometimes of one particular kind, and sometimes of another. Of this we have many instances in the Books of approved Authors, both Physitians and others;

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and I have my self observ'd some notable Examples of it. But yet I should not mention it as a ground of Suspicion, that there may be, in some times and places, unknown *Effluvia* and powers in the Air, but that I distinguih these from those Diseases of Animals, that proceed, as the Rot in Sheep often does, from the exorbitancy of the Seasons, the immoderateness of Cold, Heat, or any other manifest Quality in the Air. And you will easily perccive, that some of these Examples probably argue, that the Subterraneal parts do sometimes (especially after Earthquakes or unusual cleavings of the ground) send up into the Air peculiar kinds of venomous Exhalations, that produce new and mortal Diseases in Animals of such a *species*, and not in those of another, and in this or that particular place, and not elsewhere: Of which we have an eminent Instance in that odd Plague or Murrain of the year 1514, which *Fernelius* tells us invaded none but Cats. And even

even in Animals of the same species, sometimes one sort have been incomparably more obnoxious to the Plague than another; as *Dionysius Halicarnassus* mentions a Plague that attack'd none but *Maids*; whereas the Pestilence that raged in the time of *Gentilis* (a fam'd Physitian) kill'd few women, and scarce any but lusty Men.  
And so *Boterus* mentions a great Plague, that assaulted almost only the younger sort of persons, few past thirty years of age being attack'd by it: Which last Observation has been also made by several later Physitians. To which may be added, what Learned Men of that Faculty have noted at several times concerning Plagues, that particularly invaded those of this or that Nation, though confusedly mingled with other People; as *Cardan* speaks of a Plague at *Basil*, with which only the *Switzers*, and not the *Italians*, *French*, or *Germans*, were infected. And *Johannes Utenthorvius* takes notice of a cruel Plague at *Copenhagen*, which, though it raged among

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among the Danes, spared both the English, Dutch, and Germans, though they freely enter'd infected houses, and were not careful to shun the sick. In reciting of which Instances I would not be understood, as if I imputed these effects merely to noxious Subterraneal fumes; for I am far from denying, that the peculiar Constitutions of Mens Bodies are likely to have a great interest in them: But yet it seems less probable, that the pestilent venoma diffused through the Air should owe its enormous and fatal efficacy to the excess of the *manifest* Qualities of the Air, than to the peculiar nature of the pestilential poison sent up into the Air from under ground, which when it is by dilution or dissipation enervated, or by its progress past beyond the Air we breath in, or render'd ineffectual by subterraneal or other Corpuscles of a contrary Quality, the Plague, which it, as a con-cause, produced, either quite ceases, or degenerates into somewhat else. But I have not time

time to countenance this Conjecture, much less to consider, whether some of those Diseases, that are wont to be call'd new, which either did begin to appear, or at least to be rife, within these two or three Centuries, as the *Sudor Anglicus* in the fifteenth Century, the *Scurvy*, and the *Morbus Hungaricus*, the *Lues Moraviae*, <sup>No.</sup> *Morbus Lunehurgensis*, and some others, in the last Century of all, may be in part caus'd by the exotic steams this Discourse treats of. But this Consideration I willingly resign to Phyſitians.

And now, if the two foremen-  
tioned Suspicions, the one about *Sub-terraneal*, the other about *Sydereal*, Effluviums, shall prove to be well grounded, they may lead us to other Suspicions and further thoughts about things of no mean Consequence; three of which I shall venture to make mention of in this place.

I. For we may hence be awakened to consider, whether divers changes of Temperature and Constitution in  
the

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the Air, not only as to *manifest* Qualities, but as to the more *latent* ones, may not sometimes in part, if not chiefly, be derived from the paucity or plenty, and peculiar nature of one or both of these sorts of Effluviums. And in particular, we find in the most approved Writers such strange *Phænomena* to have several times happen'd in great Plagues and contagious Diseases, ferment'd and communicated, nay (as many eminent Physicians believed) begun, by some *latent* pestiferous, or other malignant, *Diathesis* or Constitution of the Air, as have obliged many of the Learned'st of them to have recourse to the immediate operation of the Angels, or of the Power and Wrath of God himself, or at least to some unaccountable influence of the Stars; none of the Solutions of which difficulties seems preferable to what may be gathered from our Conjecture; since of Physical Agents of which we know nothing so much, as that they are to us invisible and probably

probably of a heteroclite nature, it need be no great wonder, that the operation should also be abstruse, and the effects uncommon. And on this occasion it may be consider'd, that there are clearer inducements to persuade us, that another Quality of the Atmosphere, its *Gravity*, may be alter'd by unseen Effluviums, ascending from the Subterraneous Regions of our Globe; and we have often perceived by the Mercurial *Baroscope* the *Weight* of the Air to be notably increased, when we could not perceive in the Air nor surface of Earth any cause to which we could ascribe so notable a change. And this gives me a rise to add, that I have sometimes allowed my self to doubt, whether even the *Sun* it self may not now and then alter the *Gravity* of the Atmosphere otherwise than by its Beams or Heat. And I remember, I desired some *Virtuosi* of my acquaintance to assist me in the inquiry, whether any of the *Spots*, that appear about the Sun, may not, upon

upon their sudden dissolution, have some of their discus'd and dispers'd matter thrown off, as far as to our Atmosphere, and that copiously enough to produce some sensible alterations in it, at least as to Gravity.

H. Another thing , that our two foremention'd Suspitions, if allow'd of, will suggest , is , that it may not seem altogether improbable , that some bodies, we are conversant with, may have a peculiar disposition and fitness to be wrought on by , or to be associated with , some of those exotic Effluvia , that are emitted by unknown bodies lodged under ground , or that proceed from this or that Planet. For what we call Sympathies and Antipathies , depending indeed on the peculiar Textures and other Modifications of the bodies , between whom these friendships and hostilities are said to be exercised , I see not why it should be impossible , that there be a Cognition betwixt a body of a congruous or convenient Texture , (especially as to the shape and

and size of its Pores,) and the Effluvia of any other body, whether Subterraneal or Sydereal. We see, that convex Burning-glasses, by virtue of their figure and the disposition of their pores, are fitted to be pervaded by the beams of Light and to refract them, and thereby to kindle combustible matter; and the same beams of the Sun will impart a lucidness to the Bolonian stones. And as for Subterraneal bodies, I elsewhere mention two Minerals, which being prepared, (as I there intimate,) the steams of the one, ascending without adventitious Heat and wandering through the Air, will not sensibly work on other bodies; but if they meet with that which we prepared, they will immediately have an operation on it, whose effect will be both manifest and lasting.

III. I now pass on to the other thing, that the two formerly mentioned Suspicions may suggest, which is,

*See the Experiment  
in the Discourse of  
the Determinate  
Nature of Effluvi-  
ums.*

*Airval on  
sidereal  
m 4 n 265.*

is , that if they be granted to be well founded , we may be allow'd to consider , whether among the bodies we are acquainted with here below , there may not be found some , that may be *Receptacles* , if not also *Attractives* , of the Sydereal , and other exotic Effluviums that rove up and down in our Air .

Some of the Mysterious Writers about the *Philosophers-stone* , speak great things of the excellency of what they call their *Philosophical Magnet* , which , they seem to say , attracts and (in their phrase) corporifies the *Universal Spirit* , or (as some speak) the *Spirit of the world* . But these things being abstrusities , which the Writers of them profess'd to be written for , and to be understood only by , the *Sons of Art* ; I , who freely acknowledge I cannot clearly apprehend them , shall leave them in their own worth as I found them , and only , for brevity sake , make use of the receiv'd word of a *Magnet* , which I may do in my own sense , without

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avowing the receiv'd Doctrine of Attraction. For by such a Magnet, as I here purpose to speak of, I mean not a body that can properly attract our foreign Effluviums; but such an one, as is fitted to detain and join with them, when by virtue of the various motions, that belong to the Air as a Fluid, they happen'd to accost the Magnet. Which may be illustrated by the known way of making *Oyl of Tartar* (as the Chymists call it) *per Deliquum*. For, though the Spagyrista and others suppose, that the fiery Salt draws to it the Aqueous Vapours, yet indeed it does but arrest, and imbody with, such of those that wander through the Air, as chance in their passage to accost it.

And, without receding from the *Corpuscularian* Principles, we may allow some of the bodies, we speak of, a greater resemblance to Magnets, than what I have been mentioning. For not only such a Magnet may upon the bare account of Adhesion by

*Juxta-position or Contact*, detain the Effluviums that would glide along it, but these may be the more firmly arrested by a kind of precipitating faculty, that the Magnet may have in reference to such Effluviums; which, if I had time, I could illustrate by some Instances; nay I dare not deny it to be possible, but that in some Circumstances of time or place one of our Magnets may, as it were, *fetch in* such steams as would indeed pass near it, but would not otherwise come to touch it. On which occasion I remember, I have in certain cases been able to make some bodies, not all of them *Electrical*, attract (as they speak) without being excited by rubbing, &c. far less light bodies, than the Effluviums we are speaking of.

But this it may suffice to have glanc'd at, it not being here my purpose to meddle with the mystical Theories of the Chymists; but rather to intimate, that, without adopting or rejecting them, one may discourse

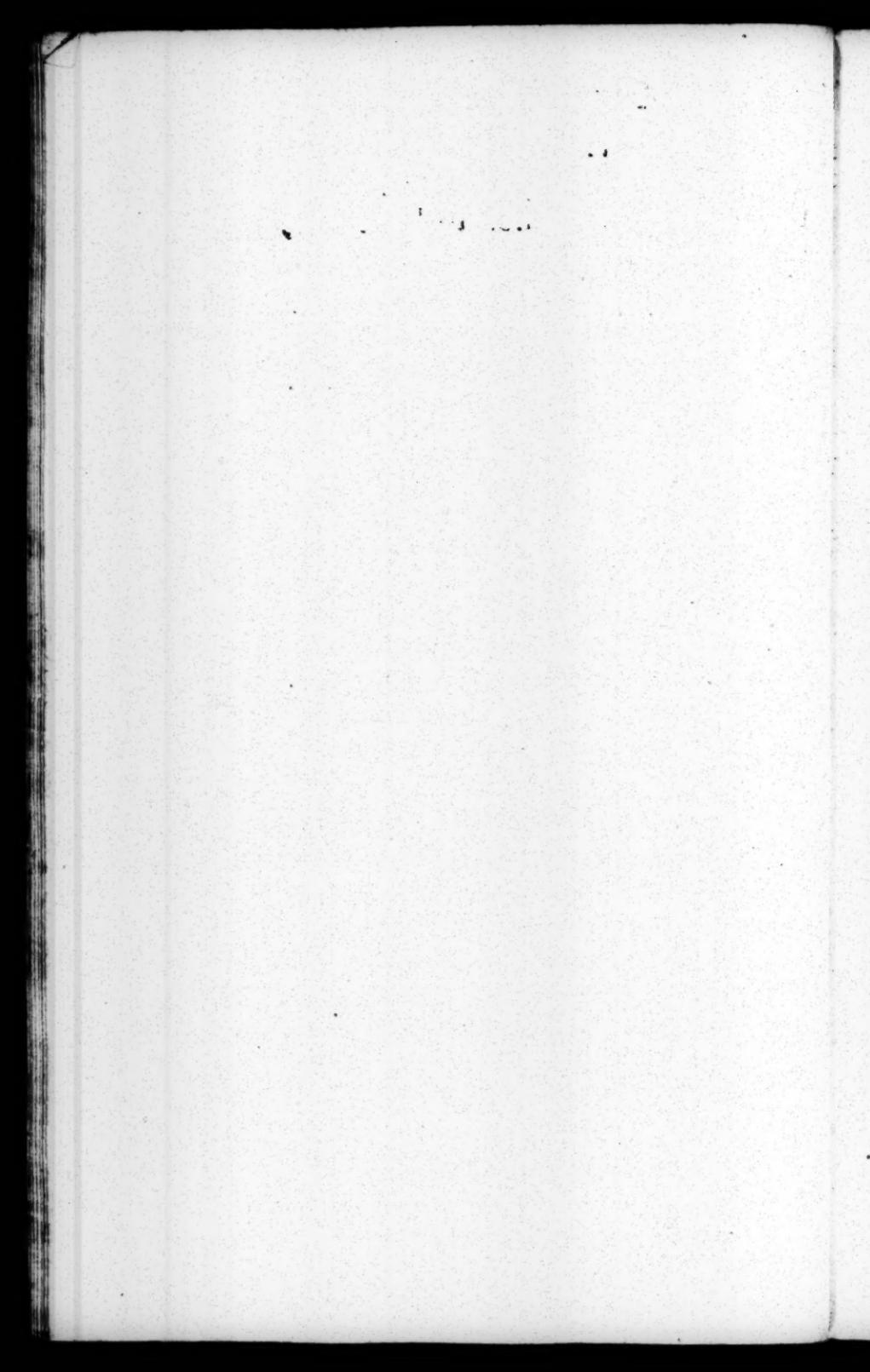
## **H**idden Qualities in the Air. 51

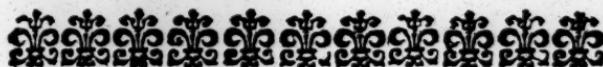
discourse like a Naturalist about Magnets of Celestial and other Emanations, that appear not to have been consider'd, not to say, thought of, either by the Scholaſtic, or even the Mechanical, Philosophers.

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O F  
CELESTIAL & AERIAL  
*MAGNETS.*

If now, upon what I have granted in the close of the past Discourse, you should urge the question further, and press me to declare, Whether, as I think it no impossible thing, that *Nature* should make, so I think it no unpracticable or hopeless thing, that *Men* should find, or *Art* should prepare, useful *Magnets* of the exotic Effluvia of the lower region of the Earth, or the upper of the World: It would much distress me to give any other answer, than that I think it extreamly difficult, and not absolutely impossible; and therefore I would not discourage any curious or

E 3      indu-

industrious Man from attempting to satisfie himself by Experiments, because even a seemingly slight discovery in a thing of this nature may be of no small use in the investigation of the nature of the Air, especially in some particular places, and of the Correspondency, which, by the intervention of the Air, the superficial part of the Terrestrial Globe may have both with the *Subterraneal* Regions of the Earth, and the *Celestial* ones of the Universe. Some of the things I have tryed or seen relating to this discovery, I must for certain reasons leave here unmention'd, and only advertise you, that several bodies, which experience has assur'd us do imbibe or retain something from the Air, as some calcin'd Minerals, some Marchasites, some Salts, as well factitious as natural, &c. may be fit to be often exposed to it, and then weighed again, and farther diligently examined, whether that which makes the increment of weight, be a meer imbibed moisture or

or also somewhat else; and likewise whether it be separable from the body or not, or however have endowed it with any considerably Quality; and if you chance to meet with a good Magnet, you may then vary Experiments with it, by exposing it long to the Air in Regions differing much in Climat, or Soil, or both, by exposing it by day only, or by night, at several Seasons of the Year, in several Temperatures of the Air, at several considerable Aspects of the Stars and Planets, by making it more or less frequently part with what it has gain'd from the Air; and in short, by having regard to variety of Circumstances, which your Curiosity and Sagacity may suggest. For, by thus diversifying the Experiment many ways, you may perhaps, by one or other of them, make some unexpected and yet important discovery of what Effluviums the Air, in particular places and times, abounds with, or wants, and perchance too, of some correspondence between the

**Terrestrial and Etherial Globes of the World.**

I shall neither be surpriz'd nor quarrel with you, if you tell me, that these are extravagant thoughts; but if I had been fortunate in preserving all, that Tryal, Observation, or other productions of some Curiosity, I once had for such Inquiries, procur'd me, you would not perhaps think me so *very* extravagant. But though I must not here make any further mention of them, and shall only take notice of one body, namely *VITRIOL*, whether crude, or unripe, and (as Chymists speak) embriinated, or Spagyrically prepar'd; yet some *Phanomena* of these Vitriolate Substances may for the present, I hope, somewhat moderate your censure for my putting you upon Observations that I fear you your self will judge unpromising, and less favourable persons than you would think phantaftical. And to let you see by a pregnant Instance, that the Air may not only have a Notable operation

tion upon Vitriol, and that, after a strong fire could work no farther on it; but that this operation was considerably diversified by Circumstances; I shall begin what I have to alledge, with what the experienc'd Zwelfer occasionally observ'd, and relates to usher in a caution about a Chymical Preparation of Vitriol: For, having inform'd his Reader, that the Colcothar, that is made by a strong Distillation, is not corrosive, he denies, that, (to use his own words) *statim à Distillatione Sal ex eodem, affusa aqua, elici queat; sed tum prius,* (continues he) *ubi aliquandiu aeri expositum fuerit; tunc enim sal præbet quandoque candidum, quandoque purpureum, aspectu pulcherrimum, quod aliquando in copia acquisivi, & penes me asservo, quandoque etiam Nitrosum.*

Which Testimony of this candid Spagyrist has much the more weight with me, because I find, what he affirms of the Saltiness of newly and strongly calcin'd Vitriol to be very agreeable to some of my Experiments

ments about *Colcothar* or blew (venereal) Vitriol; which Salt or Mineral (I mean Vitriol) is so odd a Concrete, that I have thought fit more than once to recommend the making Experiments about it to several Curious Persons, that had better opportunity to continue them than I, whose residence was not so fixt. And I remember, that one of these, a Person industrious and versed in Chymical Operations, gave me this account, that not only he had differing kinds of Salts from *Colcothar* expos'd to the Air for many months, and robb'd at convenient times of what it had acquir'd, but that in tract of time he found it so alter'd, that he obtain'd from it a pretty quantity of true running Mercury.

And now, to resume and conclude what I was saying about *Colcothar*, there are two or three things I would propose to be observed by you, or any *Virtuoso* that would assist me in these tryals about this odd *Calcinatum*, (for to call it *Terra damnata*, were to injure it.)

The

The first is, to take notice of some Circumstances that most Observers would overlook; such as (besides the Nature of the Soil) the Temperature of the Air, the Month of the Year, and the Winds, the weight of the Atmosphere, the Spots of the Sun, if any be, the Moons Age, and her Place in the *Zodiack*, and the principal Aspects of the Planets, and the other chief Stars. For, though it be a boldness to *affirm*, that any, or perhaps all of these together, will have any interest in the production of the Salt or other Substance, to be made or disclosed in the Colcothar; yet in things new and exorbitant, it may be sometimes rash and peremptory to *deny*, even such things as cannot, without rashness, be positively asserted; and in our case the small trouble of taking notice of Circumstances will be richly paid by the least discovery made in things so abstruse and considerable. And as we cannot yet knowingly pronounce, so much as *negatively*, whether the *Libration* of the

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the Moon and the Motion of the Sun (and perhaps of some of the other Planets) about their own Centers, and consequently their obverting several parts of their bodies to us, may have an operation upon our Atmosphere; so, for ought I know, there may be in those vast internal parts of the Earth, whose thin crust only has been here and there dug into by Men, considerable Masses of Matter, that may have periodical Revolutions, or Accensions, or Eftuations, or Fermentations, or, in short, some other notable Commotions, whose Effluvia and Effects may have operations, yet unobserved, on the Atmosphere and on some particular bodies expos'd to it; though these periods may be perhaps either altogether irregular, or have some kind of regularity differing from what one would expect. As we see, that the Sea has those grand Intumescencies, we call *Spring-tides*, not every day, nor at any constant day of the month or week, but about the Full and New Moon; and these Spring-

Spring-tides are most notably heigh-ten'd, not every month, but twice a year, at or about the Vernal and Autumnal Equinoxes; which Observations have not been near so antient and known, as the daily Ebbing and Flowing of the Sea. The *Etesians* of the Antients I shall not now insist on, nor the Observations that I think I elsewhere mention'd of the Elder Inhabitants of the *Caribe-Islands*, who, when the *Europeans* first resorted thither, were wont to have *Hurricanes* but once in seven years; afterwards they were molested with them but once in three years; and of late they are troubled with them almost every year. And a Physitian that lived there told me, that he had scarce ever observed them to come but within the compass of two months joyning to one another. In which Instances, and divers others that may be noted of what changes happen'd to great Quantities of Matter, Nature seems to affect something of periodical, but not in a way

way that appears to us, regular.

One may add on this occasion that memorable passage related by the

*Varenius, Lib. I. Geograph. Univers. Thermae omnes ferè quas novimus sine cessatione fluunt exceptis Piperinis Germania, &c.*

Learned Varenius of those Hot Springs in Germany, that he calls *Thermae Piperinae*, of which he af-

firms in more than one place, that they have this peculiarity, that they annually begin and cease to flow at certain times; the former about the third day of May, and the latter near the middle of September, at which time they are wont to rest till the following Spring. But though, for ought I know, our Geographers Observation will hold in hot Springs; yet it must not be extended to all, at least, if we admit that which is related by the accurate

*Johannes de Laet, (I Amer. Lib. V. cap. 7. suppose out of Ximenes, or the famous Conquerour of Mexico, Cortes,) who tells us, that in the Mexican Province, Xilotepec, Fons celebratur, qui quatuer continuis annis*

annis scaturit, deinde quatuor sequentibus deficit, & rursus ad priorem modum erumpit, & quod mirabile, pluvias diebus, parcius, quum sadum est tenpas & aridum, copiosius, exuberat.

But this is not a place to enlarge upon the grounds of my suspecting, there may be some periodical Motions and Commotions within the Terrestrial Globe; what has been mention'd being only to invite you to take notice of Circumstances in your Observations of *Colcothar*, some of which may, with the more shew of probability, be kept expos'd for a long time, because that Bars of Windows and other erected Irons I have found to acquire in tract of time from the *Effluvia* of the Earth a settled Magnetism.

The other main thing I would recommend, is, that notice be taken not only of the kind of Vitriol, the Colcothar is made of; (for I generally used blew Danzig Vitriol) as *Martial Vitriol*, *Hungarian Vitriol*, *Roman Vitriol*, &c. to which I have, for Curi-

Curiosity, added Vitriol made by our selves of the Solution of the more saline parts of Marchasites in water, without the usual additament of Iron, or Copper; *but also*, to what degree the calcination is made, and how far the calcin'd Matter is freed from the Salt by water. For these Circumstances, at least in some places, may be of moment, and perhaps may afford us good hints of the Constitution of the Atmosphere in particular parts, as well as of the best preparation of Colcothar for detaining the exotic Effluviums. And I would rather have Experiments tryed again in other places with *x* Colcothar not calcin'd to the utmost, nor yet so exquisitely edulcorated, but that some saline particles should be left in it for future increase; because I have more than once purposely tryed in vain, that the *Caput Mortuum* of blew Vitriol, whereof the Oyl and other parts had been driven off with a violent and lasting fire, would not, when fresh, impart any saltneſſ to

to the water ; nor do I think , that out of some ounces purposely edulcorated I obtained one grain of Salt. And this saltless Colcothar being expos'd , some by me , and some by a Friend that had conveniency in another place not far off , to the Air , some for many weeks and some for divers months , we did not find it to have manifestly increased in weight , or to have acquired any sensible saltiness , which , supposing the Vitriol to have nothing extraordinary , gave me the stronger suspicion of some peculiarity in the Air of that part of *London* , where the Tryals had been made , at least during those times wherein we made them ; because not only former experience , made here in *England* , had assur'd me , that some Colcothars will gain no despicable accession of weight by being expos'd to the Air ; but accidentally complaining of my lately mention'd disappointment to an ingenious Traveller , that had , in divers Countries , been curious to examine their Vitriols , he

F assured

assured me, that, though he usually dulcified his Colcothar very well, yet within four or five weeks he found it considerably impregnated by the Air twas exposed to.

3 It remains, that I add one intimation more about Vitriol, which is, that I have found it to have so great a correspondence with the Air, that it would not be amiss to try, not only Colcothar of differing Vitriols (whether barely made the common way, or without any Metalline addition to the Vitriol Stones or Ore,) but other Preparations of Vitriol too, such as exposing Vitriol, only calcin'd to whiteness by the Sun-beams, or further to an higher colour by a gentle Heat, or throughly calcined, and then impregnated with a little of its own Oyl. For such Vitriolate Substances as these, the Air may work upon, nay even liquid Preparations of Vitriol may be peculiarly affected by the Air, and thereby perhaps be useful to discover the present constitution, or foretel some approaching

ching changes of it. Of the use of which conjecture, namely the peculiar action of the Air on some Vitriolate Liquors, I remember I shew'd some *Virtuosi* a new Instance in an Experiment, whereof this was the Sum:

I elsewhere mention a Composition that I devis'd, to make with Sublimate, Copper, and Spirit of Salt, a Liquor of a Green exceeding lovely. But in the description of it I mention'd not (having no need to do it there) a circumstance as odd as the liquor it self was grateful. For the Air has so much interest in the production of this green, that when you have made the Solution of the Copper and Mercury with the Spirit of Salt, that Solution will not be *green*, nor so much as *greenish*, as long as you keep it stopt in the bolt-head, or such like glass wherein 'tis made. But if you pour it out into a Vial, which, by not being stopt, leaves it expos'd to the Air, it will after a while sooner or later attain that delightful green that so much endears it to the Beholders.

ders Eye. This appear'd so odd an Experiment to the *Virtuosi*, to whom I first related it, that those that could not guess by what means I attain'd it, could scarce believe it. But that troubled not me, who, to satisfie my self not only of the Truth of the Experiment, but that 'twas not so contingent as many others, repeated it several times, and found the Solution, 'till the *Air* made it florish, to be of a muddy reddish colour quite differing from green. So that I remember, that having once kept some of the liquor in the same glas-egg, wherein the Solution had been made, it look'd like very dirty water, whilst the other part of the same Solution, having been expos'd to the Air, emulated the colour of an Emerald. In which change 'tis remarkable, that to clarifie this liquor and give it a transparent greenes, I perceiv'd not, that any precipitation of foul matter was made to which the alteration could be ascrib'd; and yet to make it the more probable that *this* change

change proceeded not from a subsidence made of some opacating matter effected by some days rest, I kept some of the Solution seal'd up in a fine Vial several months, without finding it at the end of that time other than a dark or muddy liquor, which, in short time, it ceas'd to be, when, the Hermetic Seal being broken off, the Air was permitted to work upon it. And this I further observ'd in our various Experiments on this liquor, that, according to the quality of the matter and other Circumstances, the greenes was not attain'd to but at certain periods of time, now and then disclosing it self within two or three days, and sometimes not before nine or ten.] TRIBUS

With how little Confidence of success Tryals, that have the aimes of these I have been speaking of, are to be attempted, not only consideration but experience have made me sensible. But yet I would not discourage Mens Curiosity from venturing even upon slight probabilities,

where the Noblenes of the Subjects  
and Scope may make even small  
attainments very desirable. And  
'till tryal have been made on occa-  
sions of great moment, 'tis not easie  
to be satisfied, that Men have not been  
wanting to themselves; which I  
shall only illustrate by proposing,  
what, I presume, will not need that  
I should make an application of it.  
Those adventurous Navigators, that  
have made Voyages for Discovery  
in unknown Seas, when they first  
discern'd something obscure near the  
Horizon at a great distance off, have  
often doubted, whether what they  
had so imperfect a sight of, were a  
Cloud, or an Island, or a Mountain:  
But though sometimes it were more  
likely to be the former, as that which  
more frequently occurr'd, than the  
latter; yet they judg'd it advisable to  
steer towards it, 'till they had a  
clearer prospect of it: For if it were  
a deluding Meteor, they would not  
however sustain so great a loss in that  
of a little labour, as, in case it were  
a Coun-

a Country, they would in the loss of what might prove a rich Discovery: And if they desisted too soon from their Curiosity, they could not rationally satisfie themselves, whether they slighted a Cloud or neglected a Country.

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**F I N I S.**

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OBSERVATIONS  
ABOUT THE  
GROVVTH  
O F  
*METALS in their ORE*  
Exposed to the AIR.

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By the Honourable  
ROBERT BOYLE,  
Fellow of the Royal Society.

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LONDON,  
Printed by William Godbid, and are to  
be Sold by Moses Pitt, at the Angel over  
against the little North Door of  
St. Paul's Church. 1674,





OBSERVATIONS  
OF THE  
GROVVTH  
OF  
*M E T A L S.*

**I**T is altogether unnecessary to my present purpose, to examin, whether Metals and Minerals, as if they were a kind of subterraneal Plants, do properly grow as Vegetables do. For this Inquiry belongs to another place, but not to this, where the reference made in the 39<sup>th</sup> page of the foregoing Paper does not oblige me to speak of the *Growth of Metals* in any other than a lax and popular sense, in which a Metal may be said to grow, if a portion of

2      **Observations about the**

Matter being assign'd , wherein as yet Men can find either no Metal, as Gold or Tin , or but such a quantity of it ; this being expos'd to the Air , will after a time either afford some Metal where there appear'd none before, or a greater proportion of Metal than it had before.

Observations of this kind requiring length of time , as well as residence near places abounding with Minerals , I have little or no opportunity to make any of them my self, at least with the wariness , that to me seems due to Observations that I think not easie to be well made. And therefore I must content my self to set down what I have been able to learn by conversing with Mineralists and Travellers , and to add some particulars that I met with in Authors of good Credit.

**O B-**

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## OBSERVATIONS

ABOUT THE

### *Growth of Tin.*

**A**N ancient Owner of Mines, being asked by me, Whether he could, otherwise than upon the Conjectures of vulgar Tradition, prove, that Minerals grow even after the Veins have been dug? Answer'd affirmatively; and being desired to let me know his proofs, he gave me these that follow.

He told me, that not far from his House there was a *Tin-Mine*, which the old Diggers affirm'd to have been left off, some said eighty, some an hundred & twenty years ago, because they had by their washing and vanning separated all the Ore from the rest of the Earth, and yet of late years they

A 3 found

4 Observations about the

found it so richly impregnated with Metalline Particles , that it was wrought over again with very good profit , and preferr'd to some other Mines that were actually wrought , and had never been so robb'd . And when I objected , that probably this might proceed from the laziness and unskilfulness of Workmen in those times , who left in the Earth the Tin that was lately separated , and might then have been so ; I was answer'd , that 'twas a known thing in the Country , that in those times the Mine-men were more careful and laborious to separate the Metalline part from the rest of the Ore , than now they are .

He also affirmed to me , that in his own time some Tenants and Neighbours of his (employ'd by him) having got all the Ore they could out of a great quantity of stuff , dug out of a Tin-Mine , they laid the remains in great heaps expos'd to the Air , and within twenty and thirty years after , found them so richly im-

impregnated, that they wrought them over again with good benefit.

And lastly he assured me, that, in a Work of his own, wherein he had exercis'd his skill and experience, (which is said to be very great) to separate all the particles of the Tin from the Terrestrial substances, that were dug up with it out of the Vein, he caus'd Dams to be made to stop the Earthy Substance, which the Stream washed away from the Ore, giving passage to the water after it had let fall this Substance, which lying in heaps expos'd to the Air, within ten or twelve years, and sometimes much less, he examin'd this or that heap, and found it to contain such store of Metalline particles, as invited him to work it again and do it with profit. And yet this Gentleman was so dexterous at separating the Metalline from the other parts of Tin-Ore, that I could (not without wonder) see what small Corpuscles he would, to satisfie my Curiosity, sever from vast quantities (in propor-

6      Observations about the

tion) of Earthy and other Mineral  
stuff.

Relations agreeable to these, I re-  
ceived from another very ingenious  
Gentleman that was conversant with  
Tin-Mines, and lived not far from  
more than one of them.

I was the more solicitous to pro-  
cure an information about the Growth  
of this Metal, because the bulk of  
that, which is us'd in *Europe*, being  
found in *England*, I have met with  
little or no mention of the Growth of  
it in Outlandish Writers.

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O B-

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## OBSERVATIONS

ABOUT THE

### *Growth of LEAD.*

AS for the Growth of *Lead* in the Ore expos'd to the Air, I remember, I enquir'd about it of a Person of Quality, who had a Patent for divers Leaden Mines that were suppos'd to contain Silver, and wrought some of them himself at no small charge, yet not without profit; and, as I remember, he answer'd me, that the *Lead-Ore*, that had been wrought and laid in heaps, did, in tract of time, grow impregnated with Metal again, and, as experience manifested, became worth working a second time. And indeed some Mineralists deliver it as a general Observation, that the Growth and Renascence of Metals is more

### 8      Dissertations about the

more manifest in Lead than in any other of them. *Fessularum mons in Hetruria*, says *Boccatius Certardus*, who delivers it as a most approved Truth, *Florentiae Civitati imminens, lapides plumbarios habet, qui si excidantur brevi temporis spatio novis Incrementis restaurantur.* J. Gerhard. in Decade questionum, pag. m. 22.

*Tu subtilius ne queras* (says *Agricola*, speaking of the Growth of Mines in general) *sed tantummodo refer animum ad cuniculos, & considera, eos adeo interdum memoriam hominum in angustum venisse, ut aliqua sui parte nullum aut admodum difficultem prebeat transitum, cum eos satis latè agere soleant Fossores, ne transituros impediant.* In tales autem angustias sunt adducti propter accretiōnem materiae ex qua tapis est factus.

But whether this increment of Lead is observable in all Mines of that Metal, I was induc'd to doubt by the answer given me by a Gentleman, whose House was seated near several *Lead-Mines*, and who was himself Owner of one or two, which he yet causes

## Growth of Metals.

causes to be wrought : For this Gentleman , though a Chymist , assured me , that in the Country where he lives , which is divided by the Sea from that of the Person above-mention'd , he never observ'd the Lead-Ore to increase , either out of the Veins or in them ; but that in some places , whence Ore had been dug thirty or forty , if not fifty , years before , he perceived not on the sides of the passages , whence the Ore had been dug , that any other had grown in its place , or that the passages , though narrow before , were sensibly straighten'd , much less block'd up .

And indeed , if there were no other Arguments in the case , the straightning of the ancient passages in process of time would not convince me . For , when I consider , that the Soils that abound with Metals do usually also abound with waters , which are commonly imbibed by the neighbouring Earth ; and when I consider too , that water is somewhat expanded by being turned  
into

## 10 Observations about the

into Ice, and that this expansion is made, (as I have often tryed) though slowly, yet with an exceeding great force, by which it often stretches or breaks the Vessels that contain it: When I consider these things, *I say*, I am apt to suspect, that sometimes the increasing narrownes of the subterraneal passages in Mines may proceed from this, that the Soil that invirons them, if they lye not deep, may have the water, imbibed by them, frozen in sharp Winters. By which glaciation, the moistened portion of the Soil must forcibly endeavour to expand it self, and actually do so in the parts contiguous to the passage, since there it finds no resistance: And though the expansion made in one year or two be but small, and therefore not observed; yet, in a succession of many Winters, it may by degrees grow to be very considerable. But this suspicion I suggest not, that I would deny the Growth of Minerals, but to recommend this Argument for it to further Consideration.

And

And yet I take this to be a better proof, than what is much relied on by some Writers of Metals, who urge, that in Churches, and other magnificent Buildings, that are Leaded over, the Metalline Roofs, in a long tract of years, grow far more ponderous, in so much that often times there is a necessity to remove them, and exchange them for Brass ones. For though this plausible Argument be urged by several Writers, and among them by the Learned *Fr. Gerhardus*, pag. m. 22; yet I fear they proceed upon a Mistake. For having had some occasion to observe and inquire after this kind of Lead, I soon suspected, that the increment of weight, (which sometimes may indeed be very great) was no clear proof of the real Growth of the Metal it self. For in that which I had occasion to consider, the additional weight as well as bulk seem'd to proceed from Acetous or other Saline Corpuscles of the Timber of those Buildings, which by degrees exhaling  
and

## 12 Observations about the

and corroding that side of the Lead which they fasten'd on, turned it with themselves into a kind of Cerusse: Which suspicion I shall briefly make probable by noting, 1. That I have found by tryal purposely made, that Woods afford an acid, though not meerly acid, liquor, capable of corroding Lead. 2. That 'tis known, that Lead turned into Cerusse increases notably in weight; some say, (for I had not opportunity to try it) above six or seven in the hundred. 3. That from the Sheets of Lead that have very long cover'd Churches and the like Buildings, there is often obtain'd by scraping a good proportion of white Lead, which I have known much preferr'd by an eminent Artist to common Cerusse, when a white Pigment was to be employed. And, by the way, Mens finding this Cerusse not on that side of the Lead that is expos'd to the outward Air, (where I scarce ever observed any) but on the inside that regards the Timber and other wooden work,

work, may disabuse those that fancied this Cerusse to be a part of the Lead calcin'd by the Beams of the Sun, that strike immediately upon the Metal. And if to this it be added, that by Distillation and otherwise I have found cause to suspect, that *Alabaster* and *white Marble* may emit Spirituous parts that will invade Lead; it may be doubted, whether what *Galen* relates of the great Intumescence of Leaden bands or fastenings, wherewith the Feet of Statues were fasten'd (to their Pedestals,) be a sure Argument of the real Growth of that Metal in the Air.

But I begin to digress, and seemingly to the prejudice of the particular Scope of this Paper; but yet not to that of one of the main Scopes of all my *Physical Writings*, the Disquisition and Advancement of Truth.

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OBSERVATIONS  
ABOUT THE  
*Growth of IRON.*

I Did not find in one of our chief Mines of Iron, that there was any notice taken of the Growth of that Metal ; but in another place or two, some that deal in Iron-Ore, informed me, that they believe it grows, and may be regenerated ; and upon that account one of them set up a Work, contiguous to some Land of mine, to melt over again the remainder of Ore that had been already wrought (at a great distance from that place) and had for some Ages lain in heaps exposed to the free Air ; but with what success this chargeable Attempt has been made, I am not yet informed.

But

But of the Growth of Iron in the Island of *Ilva* or *Elva*, in the *Tyrhene Sea*, not far from the Coast of *Tuscany*, not only ancient Authors, as *Pliny* and *Strabo*, take special notice, but modern Mineralists of very good credit, as *Falopius* and *Cæsalpinus*, particularly attest the same thing; of whom the latter speaks *Lib. III. Cap. 6.* thus: *Vena ferri copio-*  
*fissima est in Italia, ob eam nobilitata,*  
*Ilva, Tyrreni Maris Insula, incredibili*  
*copia etiam nostris temporibus eam gig-*  
*nens: Nam terra, quæ eruitur dum*  
*vena effoditur, tota procedente tempore*  
*in venam convertitur.*

And the experienc'd *Agricola* gives us the like account of a place in his Country, *Germany*, In *Lygiis*, says he, *ad Sagam oppidum in pratis eruitur ferrum, fossis ad altitudinem bipedaneam actis. Id decennio renatum denuò foditur, non aliter ac Ilvæ ferrum.* *Agric. de Vet. & Nov. Met. Lib. II. Cap. 15.*

The Learned *Johan. Gerhardus*, out of a Book which he calls *Conciones Metallica*; I suppose he means the

16 Observations about the  
High-Dutch Sermons of Mathefus,  
(whose Language I understand not)  
has this notable passage to our present

J. Gerhard. Pro-  
fessor Tubingen-  
sis, Decad. Quæst.  
Physico-chymica-  
rum, pag. m. 18.

purpose: Relatum mihi  
est à metallico fassore, ad  
Ferrarias, quæ non longè  
Ambergâ distant, terram  
inanem cum ferri Minera-  
erutam, quam vocant **Den Gummer**,  
mixtam cum recrementis ferri, quæ ap-  
pellater **Der Sinder**, congestam in cu-  
mulus, instar magnicujusdam valli, soli-  
bus pluviosisque exponi, & decimo quinto  
anno denuò excoqui, eliquaque ferrum  
tante tenacitatis, ut sole lamine inde  
procudantur.

---

O B.

## OBSERVATIONS

ABOUT THE

### *Growth of SILVER.*

OF the Growth, as is supposed, of *silver* in the form of Trees or Grass or other Vegetables, I have met with some Instances among Mineralists, and I have elsewhere mention'd, that an Acquaintance of mine shew'd me a Stone, wherein he affirmed the Silver, I saw in it, to have increased since he had it. But for certain Reasons, none of these Relations seem to me very proper to my present purpose; in order to which I shall therefore set down only one Instance, which I lately met with in a *French Collection of Voyages*, publish'd by a Person of great Curiosity and Industry, (from whose

B 2 Civility

## 18. Observations about the

Civility I receiv'd the Book.) For there, in an account given by a Gentleman of his Country of a late Voyage he made to *Peru*, wherein he visited the famous Silver-Mines of *Potosi*, I found a passage which speaks to this sense: *Le meilleur Argent, &c.*

*Voyage du Sieur au Peru, pag. 15.* i. e. The best Silver in all the Indies and the purest is that of the Mines of *Potosi*; the chief have been found in the Mountain of *Aranzasse*: And, (some Lines being interpos'd) 'tis added, that they draw this Metal even from the Mineral Earths that were in times past thrown aside, when the ground was open, and the Groves and Shafts that are in the Mountains were made; it having been observ'd that in these recrements Metal had been formed afresh since those times, which sufficiently shews the propensity of the Soil to the production of this Metal; yet 'tis true, that these impregnated Earths yield not so much as the ordinary Ore which is found in Veins betwixt the Rocks.

O B-

*OBSERVATIONS  
ABOUT THE  
Growth of GOLD.*

**A**S for the Growth of *Gold*, the Enquiries I have yet made among Travellers give me no great satisfaction about it, and though I have spoken with several that have been at the Coast of *Guiny*, and in *Congo*, and other Parts of *Afric*, where much Gold is to be had; yet I could not learn by them, that they, or any Acquaintance of theirs among the Natives, had seen any Mines or Veins of *Gold*, (which yet divers Authors affirm to be found in more than one Kingdom in *Ethiopia*, and in some other *African Countries*.) And having afterwards met with a Learned Traveller, that had carefully vi-

## 30 Observations about the

sited the famous Gold-Mine of *Cremnitz* in *Hungary*, he answer'd me, That he did not learn from the Miners, whether or no the Ores of Gold, &c. did really grow or were regenerated in tract of time, by being expos'd to the Air, or upon any other account; but the Grand Overseer, who was Lord of part of the Soil, told him, that he thought the whole Mountain to abound with particles of Gold, and therefore was wont, when the Diggers had almost exhausted the Vein, to cast-in store of Earth, and dig up other neighbouring places, which, being kept there as in a Conservatory, would afterwards afford Gold, as the Mine did before.

And, if a late German Professor of Physic do not misinform us, his Country affords us an eminent Instance of the Growth or Regeneration of Gold.

Johan. Gerhardus in Decade  
Quæstionum pag. m. 19. *Nam Corbachi*, says he,  
*qua est Civitas Westphalia*  
*sub ditione Comitis de I-*  
*senborg & Waldeck, Au-*  
*rum excoquitur ex cumulis congestis, ita*

ut singulis quadrienniis iterum elabore-  
tur cumulus unus, semper se restaurante  
natura, &c.

POSTSCRIPT.

SINCE the setting down of the foregoing Observations, I casually met with a curious Book of Travels, lately made by the very Ingenious Dr. Edward Brown, and finding in pag. 100. a couple of Relations, that seem pertinently referable, the one, to a passage above-cited out of *Agricola*, in the Notes about the Growth of Lead, and the other to the present Title about the Growth of Gold; I thought fit to annex them in the Learned Authors own words, viz.

1. Some passages in this Mine cut through the Rock, and long disus'd, have grown up again: And I observed the sides of some, which had been formerly wide enough, to carry their Ore through;

to approach each other, so as we passed with difficulty. This happens most in moist places; the passages unite not from the top to the bottom, but from one side to another.

2. The common yellow Earth of the Country near Cremnitz, especially of the Hills towards the west, although not esteem'd Ore, affords some Gold: And in one place, I saw a great part of an Hill digg'd away, which hath been cast into the Works, washed and wrought in the same manner as pounded Ore, with considerable profit.

---

The foregoing Observations about the Growth of Gold and other Metals are not all that I might, perhaps without being blamed for it, have referr'd to that Title. But all my Papers, wherein other Observations of this kind were set down, are not now at hand, and divers other Instances, that I have met with among Wri-

Writers of the Growth of Metals, (taking that expression in the sense I formerly declared) do not seem to me so pertinent in this place, because the improving Ores were not expos'd, nor perchance accessible, to the Air. And even as to the Instances that I have now mention'd, 'till severer Observations have been made, to determin whether it be partly the contact or the operation of the Air, or some internal disposition, analogous to a Metalline Seed or Ferment, that causes this Metalline Increment, I dare not be positive; though I thought the Interest of the Air in this Effect might make it pardonable, to add on this occasion to the History of Nature some particulars, of which the Cause conjecturally proposed may be probable enough to countenance a Suspicion, 'till further Experience have more clearly instructed us.

To what has been said of the Growth of Metals in the Air, I might add some Instances of the Growth

Growth of Fossile Salts, and of some other Minerals : But , besides that these belong to the Paper about the Saltinesses of the Air ; what has been already said may suffice for the present occasion.

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## POSTSCRIPT.

**A**fter what I writ in the 23<sup>th</sup> page of the foregoing Discourse, having an opportunity to look again upon the Marchasite there mention'd to have been Hermetically seal'd up after its surface had been freed from the grains of Vitriolate Salt that adher'd to it , I perceiv'd , that, notwithstanding the Glass had been so closely stopp'd , yet there plainly appear'd from the outside of the mass some grains of an Efflorescence , whose colour , between blew and green , argued it to be of a Vitriolate nature . If this be seconded with other trials made

made with the like success, it may suggest new thoughts about the *Growth of Metals* and Minerals, especially with reference to the Air.

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**F I N I S.**

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SOME ADDITIONAL  
**EXPERIMENTS**  
Relating to the  
**SUSPICIONS**  
*ABOUT THE*  
**HIDDEN QUALITIES**  
of the AIR.

---

By the Honourable  
**ROBERT BOTTLE,**  
Fellow of the Royal Society.

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**LONDON,**  
Printed by William Godbid, and are to  
be Sold by Moses Pitt, at the Angel over  
against the little North Door of  
St. Paul's Church. 1674.





SOME ADDITIONAL  
**EXPERIMENTS**

Relating to the  
**SUSPICIONS**  
 ABOUT THE  
*Hidden Qualities of the AIR.*

The ESSAY about *Suspicions*  
 of some hidden *Qualities of*  
 the *Air*, having been de-  
 tain'd somewhat long at  
 the Pres', that it might come abroad  
 accompanied with the other Tracts  
 design'd to attend it, whilst I was

A 2                  ruma-

## 2 ADDITIONAL EXPERIMENTS.

rumaging among several Papers to look for some other things, I met now and then with an Experiment or Observation, that seem'd to relate to some of the things deliver'd in that *Tract*; and though they be in themselves of no great moment, I am content to annex them to the rest, because, as in that company they may signify somewhat, so I am unwilling that any matter of fact, relating to such a Subject, should perish to save the labour of transcribing.

### EXPER. I.

Having occasion to dulcifie some *Calx* of *Dantzig-Vitriol*, from which the Oil had been a good while before distill'd; water was put upon two large portions of it, that the liquor might be impregnated with the Vitriolate particles remaining in the *Calx*; the water put upon one of these portions was, soon after it was sufficiently

ciently impregnated, filtrated and gently abstracted, by which means it afforded many drams of a kind of Salt of Vitriol that seem'd to differ very little from the Vitriol that had been calcin'd: But the water that was put upon the other portion of calcin'd Vitriol, was in a wide-mouth'd vessel left in the Air for a month or six weeks; after which time, when it came to be abstracted after the manner formerly recited, it afforded many drams of a Salt that did not then, nor long after, look at all like common Vitriol, or like the other, but shot white almost like Salt-petre, or some other untincted Salt. Whether this Experiment will constantly succeed, and at other Seasons of the Year than that 'twas made in, which was Summer, I had not the opportunity to make a full trial, though I endeavour'd it. But that the Air may have a great stroke in varying the Salts obtainable from calcin'd Vitriol, seem'd the more probable, be-

A 3 cause

## 4 ADDITIONAL EXPERIMENTS.

cause we had some Colcothar that had lain many months, if not some years, in the Air, but in a place shelter'd from the Rain; and having caus'd a *lixivium* to be made of it, to try what sort or plenty of Saline particles it would yield, we found, when the superfluous moisture was exhaled, that they began to shoot into Salt far more white than Vitriol, and very differing from it in its figure and way of Concretion.

## EXPER. II.

We took Colcothar of Venereal Vitriol carefully dulcified, and leaving it in my Study in the <sup>this was made at Oxford.</sup> Month of January and February, by weighing it carefully before an ounce of it was expos'd to the Air, and after it had continued there some weeks, we found it to have increas'd in weight four grains and about a quarter, besides

sides some little dust that stuck to the Glas.

This slight Experiment is here mention'd, that, being compar'd with the next ensuing Trial, it may appear, that the difference of Airs, Seasons, *Caleses* of Vitriol, or other Circumstances, may produce a notable disparity in the Increment of weight, the exposed Bodies gain in the Air.

## EXPER. III.

We put eight ounces of Outlandish Vitriol, calcin'd to a deep rednes, into a somewhat broad and flat Metalline vessel, and set it by upon a shelf, in a Study that was seldom frequented; and at the same time, that we might observe what increment would be gain'd by exposing to the Air a larger *superficies* of the powder in reference to the bulk, we put into another Metalline vessel, smaller than the other, only two ounces of Col-

A 4 cothar,

## 6 Additional Experiments.

cotliar, and set it on the same shelf with the other, this was done at the Vernal Equinox, (the Twelfth of March;) on the twenty fifth of June we weigh'd these powders again, and found the eight ounces to have gained one dram and seventeen grains; but the two ounces had acquired the same weight within a grain: Then putting them back into their former vessels, we left them in the same place as formerly, 'till the twenty fourth of August, when we found cause to suppose, that the greater parcel of Colcothar had met with some mischance, either by Mice or otherwise; but the lesser parcel weigh'd Twenty six grains heavier than it did in June, amounting now to two ounces, one dram, forty two grains, having increased, in les than six months, above an hundred grains, and consequently above a tenth part of its first weight.

No Trial was made to discover what this acquir'd Substance may be,

be, that we might not disturb the intended prosecution of the Experiment.

**EXPER. IV.**

Because in most of the Experiments of Substances expos'd to be impregnated by the Air, or detain its Saline or other exotic particles, we employed Bodies prepar'd and much alter'd by the previous operation of the Fire; we thought fit to make some Trials with Bodies unchanged by the Fire, and to this purpose we took a Marchasite, which was partly of a shining and partly of a darkish colour, and which seem'd well dispos'd to afford Vitriol; of this we took several smaller Lumps, that amounted to two ounces; these were kept in a room, where they were freely accessible to the Air, which, by reason that the House, that was seated in the Country, stood high, was

was esteemed to be very pure. After the Marchasites had been kept in this room somewhat less than seven weeks, we weigh'd them again in the same Ballance, and found the two ounces to have gained above twelve grains in weight.

REQUIR'D OF THE FLOWERS OF SWEET-ROSE  
MILKED OR BACON & ASHES, AND TO AYD  
IN THIS JOB TO EXPER. V. A BOTTLE OF  
WINE, COULDING OINTMENT, & SOUL'D  
DO THE EXPERIMENT US'D AT THE LATTER  
END OF OUR PAPER, ABOUT CELESTIAL AND  
TERRESTRIAL MAGNETS, SEEMING TO SOME  
VIRTUOSI VERY STRANGE; AND THE WAY  
THAT I EMPLOY'D IN MAKING THAT LI-  
QUOR, THAT TURNS GREEN IN THE AIR,  
BEING SOMEWHAT TROUBLE-SOME, I RE-  
MEMBER I THOUGHT FIT TO TRY UPON  
THE SAME GROUND A WAY OF PRODUCING  
THE SAME PHÆNOMENON MORE EASIE AND  
MORE EXPEDITIOUS. AND THOUGH PER-  
HAPS THIS WAY WILL NOT SUCCEED SO CON-  
STANTLY, NOR ALWAYS SO WELL AS THE  
OTHER, YET FOR ITS EASINESS AND CHEAPNESS  
IT WILL NOT PROBABLY BE UNWELCOME

to those that are desirous to see the odd Phænomenon.

We took then, more than once, filings of clean crude Copper, and having put on them a convenient quantity of good Spirit of Salt, we suffer'd the *Menstruum in Heat* (which need not be very great) to work up on the Metal, which it usually does slowly, and not like *Aqua-fortis*: When the Liquor had by this operation acquir'd a thick and muddy colour, we decanted it into a clean Glass with a wide mouth, which being left for a competent time in the open Air, the exposed Liquor came to be of a fair green, though it did not appear that any thing was precipitated at the bottom, to make it clear.

## EXPER. VI.

Perhaps it may not be impertinent to add, that I once or twice observ'd the fumes of a sharp Liquor to work more quickly or manifestly on a certain Metal sustained in the Air, than did the *Menstruum* it self that emitted those fumes on those parts of the Metal that it cover'd: And this brings into my mind, that, asking divers Questions of a Chymist that had been in *Hungary* and other parts, purposely to see the Mines; he answer'd me, among other things, that, as to the Ladders and other wooden work employed in one or more of the deep *Hungarian* Mines, those that were in the upper part of the Groves any thing near the external Air, would by the fretting Exhalations be render'd unserviceable, in not many months, whereas those Ladders and pieces of Timber, &c. that were employed in the lower part of

## **Additional Experiments.** 11

of the Mine, would hold good for two or three times as long.

### *EXPER. VII.*

We took about the bigness of a Nutmeg of a certain soft but consistent Body, that we had caus'd to be Chymically prepared, and which in the free Air would continually emit a thick smoak: This being put into a Vial, and placed in a middle sized Receiver in our Engin, continued for some time to afford manifest fumes, whilst the exhaustion was making; 'till at length, the Air having been more and more pump'd out, the visible ascension of fumes out of the Vial quite ceas'd, and the matter having remain'd some time in this state, the smoaking substance was so alter'd, that it would not emit fumes, not only when the Air was let into the Receiver, but not in a pretty while after the Vial was taken out

## 12 Additional Experiments.

out of it, 'till it had been removed to the window, where the Wind blowing-in fresh and fresh Air, it began to smoak as formerly.

The other *Phenomena* of this Experiment belong not to this place; but there are two, which will not be impertinent here, and the latter of them may deserve a serious Reflection.

The *first* of them was, that the Substance hitherto mention'd had been kept in a large Glass, whereinto it had been distill'd at least five or six weeks, and yet would smoak very plentifully upon the contact of the Air, and be kept from smoaking, though the Chymical Receiver were stopp'd but with a piece of paper.

The *second* was, that, when the Vial was put unstopp'd in the Receiver, and the Receiver close luted on, though no exhaustion were made, yet the white fumes did very quickly cease to ascend into the Receiver, as if this Smoak participated

of

of the nature of Flame , and pre-  
sently glutted the Air , or otherwise  
made it unfit (and yet without dimi-  
nution of its gravity) to raise the Bo-  
dy that should ascend.

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**F I N I S.**

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*ANIMADVERSIONS*  
UPON  
MR. HOBBE'S  
*PROBLEMATICA*  
DE  
*VACUO.*

---

By the Honourable  
ROBERT BOYLE,  
Fellow of the Royal Society.

---

LONDON,  
Printed by William Godbid, and are to  
be Sold by Moses Pitt, at the Angel over  
against the little North Door of  
St. Paul's Church. 1674.

МАНДАРСИО

ион

Л. ХОББЕС

ФРАНКЛІН

ОДЕАВ

одиннадцять



БІБЛІОТЕКА

ІМЕНІ А. С. ПАСКАЛЯ

ІІІ



## PREFACE.

UPON the coming abroad of Mr. Hobbes's Problemata Physica, finding them in the hands of an Ingenious Person, that intended to write a Censure of them, which several Employments private and publick have, it seems, hinder'd him to do; I began, as is usual on such occasions, to turn over the leaves of the Book, to see what particular things it treated of. This I had not long done before I found, by obvious passages in the third Chapter, or Dialogue, as well as by the Title, whitch was Problemata de Vacuo, that I was particularly concern'd in it; upon which I desired the Possessor of the Book, who readily consented, to leave me to examin that Dialogue, on which condition I would leave him to deal with all the rest of the Book. Nor did I look upon the Reflections I meant to make as repugnant

a\*

to

## P R E F A C E.

to the Resolutions I had taken against writing Books of Controversie, since the Explications, Mr. Hobbes gave of his Problems, seem'd to contain but some Variations of, or an Appendix to, his Tract De Natura Aeris , which , being one of the two first pieces that were published against what I had written , was one of those that I had expressly reserv'd myself the liberty to answer. But the Animadversions I first made upon Mr. Hobbes's Problems De Vacuo , having been casually mislaid e're they were finished; before I had occasion to resume my task , there past time enough to let me perceive, that his Doctrine, which 'twill easily be thought that the Vacuists disapproved, was not much relished by most of the Plenists themselves , the modernest Peripateticks and the Cartesians ; each of them maintaining the Fullness of the World, upon their own grounds, which are differing enough from those of our Author, the natural Indisposition I have to Polemical Discourses , easily perswaded me to let alone a Controversie, that did not appear needful: And I had still persisted

## PREFACE.

in my silence, if Mr. Hobbes had not  
as 'twere summon'd me to break it by  
publishing again his Explications, which  
in my Examen of his Dialogue De Na-  
tura Aeris I had shewn to be erroneous.

And I did not grow at all more satisfied,  
to find him so constant as well as stiff an  
Adversary to interspers'd Vacuities, by  
comparing what he maintains in his Dia-  
logue De Vacuo, with some things that  
he teaches, especially concerning God, the  
Cause of Motion, and the Imperviousness  
of Glass; in some other of his Writings  
that are published in the same Volume  
with it. For since he asserts that there  
is a God, and owns Him to be the Creator  
of the World; and since on the other side  
the Penetration of Dimensions is confessed  
to be impossible, and he denies that there  
is any Vacuum in the Universe; it seems  
difficult to conceive, how in a world that  
is already perfectly full of Bodie, a Cor-  
poreal Deity, such as he maintains in his  
Append. ad Leviath. cap. 3, can have  
that access even to the minute parts of  
the Mundane Matter, that seems requi-  
site to the Attributes and Operations that

## PREFACE.

belong to the Deity, in reference to the world. But I leave Divines to consider what Influence the conjunction of Mr. Hobbes's two Opinions, the Corporeity of the Deity, and the perfect Plenitude of the world, may have on Theology. And perhaps I should not in a Physical Discourse have taken any notice of the proposed Difficulty, but that, to prevent an Imputation on the Study of Natures Works, (as if it taught us rather to degrade than admire their Author,) it seem'd not amiss to hint (in transitu) that Mr. Hobbes's gross Conception of a Corporeal God, is not only unwarranted by sound Philosophy, but ill befriended even by his own.

My Adversary having propos'd his Problems by way of Dialogue between A. and B; 'twill not, I presume, be wonder'd at, that I have given the same form to my Animadversions; which come forth no earlier, because I had divers other Treatises, that I was more concern'd for, to publish before them.

But because it will probably be demanded, why on a Tract that is but short, my

## PREFACE.

my Animadversions should take up so much room? It will be requisite, that I here give an account of the bulk of this Treatise.

And first, having found that there was not any one Problem, in whose Exposition, as propos'd by Mr. Hobbes, I saw cause to acquiesce, I was induc'd for the Readers ease, and that I might be sure to do my Adversary no wrong, to transcribe his whole Dialogue, bating some few Transitions, and other Clauses not needful to be transferr'd hither.

Next, I was not willing to imitate Mr. Hobbes, who recites in the Dialogue we are considering the same Experiments that he had already mentioned in his Tract De Natura Aeris, without adding as his own (that I remember) any new one to them. But my unwillingness to tire the Reader with bare

Credo, (says Mr. Hobbes in his Dialogus Physico-Aeris, p. 13. ) Nam hic Restitutionis, Hobbit est, & ab illo primo & solo explicatus in Lib. de Corpore, cap. 21. Art. 1. Sine qua Hypothesi, quantus cunque labor, ars, sumptus, ad rerum Naturalium invisibilis causas inveniendas adhibeat, frustra erit. And speaking of the Gentlemen (to whom it were not here proper for me to give

## PREFACE.

give Epithets) that we'd  
so met at Gresham-  
College, and was known  
by the Name of the  
Royal Society, he then  
tears them, and their  
way of inquiring into  
Nature & Conveniencie,  
studia concept, Experi-  
menta faciant quantum  
volunt, nisi &  
Principiis stantur me-  
is, nihil proficiunt.

A. Fateris ergo ni-  
hil doctenus a Collegis  
quis promotam esse sci-  
entium Canalarum Na-  
turalium, nisi quod Ul-  
pus eorum Machinam  
invenerit, qua motus  
excita Aetis posse tal-  
lis, ut partes Sphaere  
simil undique ten-  
dant ad Centrum, &  
ut Hypotheses Hobbes-  
ianae, ante quidem satis  
probabiles, hinc red-  
dantur probabiliore.

B. Nec fateri pu-  
det; nam est aliquid  
prodire temus, si non  
datur ultra.

C. Quid ~~autem~~?  
quorum autem tantus  
apparatus & sumptus  
Machinarum factu dif-  
ficilium, ut easenus tan-  
tum prediretis quantu-

bare Repetitions of the  
Arguments I employ'd  
in my Examen of that  
Tract, invited me to  
endeavour to make  
him some amends for  
the exercise of his pa-  
tience by inserting,  
as occasion was offer'd,  
five or six new Experi-  
ments, that will not  
perhaps be so easily  
made by every Reader  
that will be able (now  
that I have perspic-  
uously propos'd them) to  
understand them.

And lastly, since  
Mr. Hobbes has not  
been content to mag-  
nifie himself and his  
way of treating of  
Physical matters, but  
has been pleas'd to  
speak very slighting-  
ly of Experimental  
Philosophers (as he  
stiles

## PREFACE.

files them) in general, and, which is worse, to disparage the making of elaborate Experiments; I judg'd the thing, he seem'd to aim at, so prejudicial to true and useful Philosophy, that I thought, it might do some service to the less knowing, and less wary, sort of Readers, if I

tryed to make his own Explications enervate his Authority, and by a somewhat particular Examen of the Solutions he has given of the Problems I am concern'd in, shew, that 'tis much more easie to undervalue a frequent recourse to Experiments, than truly to explicate the Phænomena of Nature without them. And since our Author, speaking of his Problemata Physica, (which is but a small Book) scruples not to tell His Majesty, to whom he dedicates them, that he has therein comprised (to speak in his own terms) the greatest and most probable

part

tum ante pridem Hobbius? Cur non inde potius incepisti ubi ille desit? Cur Principiis ab illo positis non estis tisi? Cumque Aristoteles recte dixit, ignoratio motu ignorari Naturam, &c.

— Ad Causas autem, propter quas proficere ne paululum quidem potuisti, nec poteritis, accedunt etiam aliae, ut odium Hobpii, &c.

## PREFACE.

part of his Physical Meditations; and since by the alterations, he has made in what he formerly writ about the Phænomena of my Engine, he seems to have design'd to give it a more advantageous form: I conceive, that by these selected Solutions of his, one may, without doing him the least injustice, make an estimate of his way of discoursing about Natural things. And though I would not interress the credit of Experimentarian Philosophers in no considerabler a Paper than this; yet if Mr. Hobbes's Explications and mine be attentively compared, it will not, I hope, by them be found, that the way of Philosophising he employs, is much to be preferr'd before that which he under-values.

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ANI-

C I T Y

A N I M A D V E R S I O N S

U P O N

M<sup>r</sup>. H O B B E S<sup>s</sup>

P r o b l e m a t a d e V a c u o.

A. Ay me, without too bold an inquisitiveness, ask, what Book you are reading so attentively?

B. You will easily believe you may, when I shall have answer'd you, that 'twas Mr. Hobbes's lately publish'd Tract of Physical Problems, which I was perusing.

A. What progress have you made in it?

B. I was finishing the third Dialogue or Chapter when you came in,

A 2 and

2 **Animadverſions upon**

and finding my ſelf, though not na-  
med, yet particularly concern'd, I  
was perufing it with that attention  
which it ſeems you took notice of.

**A.** Divers of your Experiments  
are ſo expreſly mention'd there, that  
one need not be ſkill'd in decyphering  
to perceive that you are intereffed in  
that Chapter, and therefore ſeeing  
you have heedfully read it over, pray  
give me leave to ask your Judgment,  
both of Mr. Hobbeſ's Opinion, and his  
Reaſonings about *Vacuum*.

**B.** Concerning his Opinion, I am  
ſorry I cannot now ſatisfie your Cu-  
riosity, having long ſince taken, and  
ever ſince kept, a Resolution to de-  
cline, at leaſt until a time that is not  
yet come, the declaring my ſelf ei-  
ther for or againſt the *Plenifts*. But  
as to the other part of your Question,  
which is about Mr. Hobbeſ's Argu-  
ments for the absolute Plenitude of  
the World, I ſhall not ſcruple readily  
to anſwer, that his Ratiocinations  
ſeem to me far ſhort of that cogency,  
which the noife he would make in  
the

the world , and the way wherein he treats both ancient and modern Philosophers that dissent from him , may warrant us to expect .

A. You will allow me the freedom to tell you , That , to convince me , that your resentment of his expounding divers of the Phænomena of your Pneumatic Engine otherwise than you have been wont to do , (and perhaps in terms that might well have been more civil,) has had no share in dictating this Judgment of yours ; the best way will be , that entering for a while into the party of the *Vacuists* you answer the Arguments he alledges in this Chapter to confute them .

B. Having always , as you know ; forborn to declare my self either way in this Controversie , I shall not tye my self strictly to the Principles and Notions of the *Vacuists* , nor , though but for a while , oppose my self to those of the *Plenists* : But so far I shall comply with your Commands , as either upon the Doctrine of the *Vacuists* , or upon other grounds , to consider , whe-

4 *Animadversions upon*

ther this Dialogue of Mr. Hobbes have cogently proved his, and the Schools, Assertion, *Non dari Vacuum*; and whether he has rightly explain'd some Phænomena of Nature which he undertakes to give an account of, and especially some produced in our Engin, whereof he takes upon him to render the genuine Causes. And this last inquiry is that which I chiefly design.

A. By this I perceive, that if you can make out your own Explications of your Adversaries Problems *de Vacuo*, and shew them to be preferable to his, you will think you have done your work, and that 'tis but your seconday scope to shew, that in Mr. Hobbes his way of solving them, he gives the *Vacuists* an advantage against *Him*, though not against the *Plenists* in general.

B. You do not mistake my meaning, and therefore without any further Preamble, let us now proceed to the particular Phænomena consider'd by Mr. Hobbes; the first of which is an Experiment proposed by me in the one

one and thirtieth of the *Physico-Mechanical Experiments* concerning the Adhesion of two flat and polish'd Marbles, which I endeavour'd to solve by the pressure of the Air. And this Experiment Mr. Hobbes thinks so convincing an one to prove the Plenitude of the World, that, though he tells us he has many cogent Arguments to make it out, yet he mentions but this one, because that, *he says,* suffices.

A. The Confidence he thereby expresses of the great force of this Argument does the less move me, because, I remember, that formerly in his *Elements of Philosophy* he thought it sufficient to employ one Argument to evince the Plenitude of the World, and for that one he pitch'd upon the Vulgar Experiment of a Gardeners Watering-Pot : But, whether he were wrought upon by the Objections made to his Inference from that Phænomenon in your Examen of his Dialogue *De Natura Aeris*, or by some other Considerations, I will not pretend to divine. But I plainly perceive, he

6. *Animadversions upon*

now prefers the Experiment of the cohering Marbles.

B. Of which it will not be amiss, though the passage be somewhat long, to read you his whole Discourse out of the Book I have in my hand.

A. 'Tis fit that you, who for my sake are content to take the pains of answering what he says, should be eased of the trouble of reading it, which I will therefore, with your leave, take upon me. His Discourse then about the Marbles is this :

A. *Ad probandam Universi Plenitudinem, nullum nostrin' Argumentum cogens?*

B. *In* multa : *Unum autem sufficit ex eo sumptum, Quod duo corpora plana, si se mutua secundum amborum planitatem communem tangant, non facile in instante diuelli possunt; successivè vero facillime. Non dico, impossibile esse duo durissima Marmora ita coherentia diuelleret, sed difficile; & vim postulare tantam, quanta sufficit ad duritatem lapidis superandam. Siquidem vero majore vi ad separationem opus sit quam illa, qua moven-*

M. Hobbes's Problemata de Vacuo. 7  
moventur separata, id signum est non  
dari Vacuum.

A. Assertiones illae demonstratione  
indigent. Primo autem ostende, quomo-  
do ex duorum durissimorum corporum,  
coniunctorum ad superficies exquisite la-  
ves, diremptione difficulti, sequatur Ple-  
nitudo Mundi?

B. Si duo plana, dura, polita Cor-  
pora (ut Marmora) collocentur unum su-  
pra alterum, ita ut eorum superficies se-  
mutuò per omnia puncta exactè, quan-  
tum fieri potest, contingant, illa sine  
magna difficultate ita divelli non pos-  
sunt, ut eodem instante per omnia pun-  
cta dirimantur. Veruntamen Marmora  
eadem, si communis eorum superficies ad  
Horizontem erigatur, aut non valde in-  
clinetur, alterum ab altero facillime  
(ut scis) etiam solo pondere dilabentur.  
Nonne causa hujus rei hac est, Quod  
labenti Marmori succedit Aer, & reli-  
ctum locum semper implet?

A. Certissime. Quid ergo?

B. Quando vero eadem uno instante  
divellere conaris, nonne multo major vis  
adhibenda est; Quam ob causam?

A. Ego,

A. Ego, & mecum (proto) omnes canſam ſtatuant, Quod ſpatium totum inter duo illa Marmora diuulſa, ſimul uno iuſtante implere Aer non potest, quanta cunque celeritate fiat diuulſio.

B. An qui ſpatia in Aere dari vacua contendunt, in illo Aere ſolo dari negant qui Marmora illa coniuncta circumdat?

A. Minime, ſed ubique interſpersa.

B. Dum ergo illi, qui Marmor umum ab altero revellentes Aerem comprimunt, & per conſequens Vacuum exprimunt, Vacuum faciunt locum per reuulſionem relictum; nulla ergo ſeparationis erit difficultas, ſaltem non major quam eft difficultas corpora eadem movendi in Aere poſtquam ſeparata fuerint. Itaque quoniam, confeſſo Vacuum, difficultas Marmora illa dirimendi nulla eft, ſequitur per difficultatis experientiam, nullum eſſe Vacuum.

A. Reſte quidem illud infers. Mundus autem Plenitudine ſappofita, quomodo demonſtrabis poſſibile omnino eſſe ut diuellanter?

B. Cogita primo Corpus aliquod diuile,

etile, nec nimis durum, ut ceras, in duas partes distrahi, que tamen partes non minus exacte in communi piano se mutuo tangunt quam levissima Marmora. Nam quo pacto distrahitur cera, consideremus. Nonne perpetuo attenuatur donec in filum evadat tenuissimum, & omni dato crasso tenuius, & sic tandem divellitur? Eodem modo etiam durissima columna in duas partes distrahetur, si vim tantam adhibetur, quanta sufficit ad resistentiam duritiae superandam. Sicut enim in cera partes primò extime distrahuntur, in quarum locum succedit Aer; ita etiam in Corpore quantumlibet duro Aer locum subit partium extimarum, quæ prima Vulsione viribus dirumpuntur. Vis autem quæ superat resistentiam partium extimarum Duri, facile superabit resistentiam reliquarum. Nam resistentia prima est à Toto Duro, reliquarum vero semper à Residuo.

A. Ita quidem videtur consideranti, quæ Corpora quedam, præsettim vero durissima, fragilia sint.

Does this Ratiocination seem to you as cogent, as it did to the Proposer of it?

B. You

B. You will quickly think it does not, and perhaps you will think it should not, if you please to consider with me some of the Reflections that the Reading of it suggested to me,

And first, without declaring for the *Vacuists* Opinion, I must profess my self unsatisfied with Mr. Hobbes's way of arguing against them: For, where he says, *Dum ergo illi qui Marmor unum ab altero revellentes Aerem comprimunt & per consequens Vacuum exprimunt, Vacuum faciunt locum per revolutionem relictum; nulla ergo separationis erit difficultas, saltem non major quam est difficultas corpora eadem movendi in Aere postquam separata fuerint.* Itaque quoniam, concessso Vacuo, difficultas Marmora illa dirimendi nulla est, sequitur per difficultatis experientiam, nullum esse Vacuum. Methinks he expresses himself but obscurely, and leaves his Readers to gheſſ, what the word *Dum* refers to. But that which seems to be his drift in this passage, is, that, since the *Vacuists* allow interſpersed Vacuities, not only in the Air

that

that surrounds the conjoyned Marbles, but in the rest of the ambient Air, there is no reason, why there should be any difficulty in separating the Marbles, or at least any greater difficulty than in moving the Marbles in that Air after their separation. But, not to consider, whether his Adversaries will not accuse his phrase of *squeezing out a Vacuum* as if it were a Body, they will easily answer, that notwithstanding the Vacuities they admit in the ambient Air, a manifest reason may be given in their Hypothesis of our finding a difficulty in the Divulsion of the Marbles. For, the Vacuities they admit being but interspers'd, and very small, and the Corpuscles of the Atmosphere being according to them endow'd with Gravity, there leans so many upon the upper surface of the uppermost Marble, that that stone cannot be at once perpendicularly drawn up from the lower Marble contiguous to it, without a force capable to surmount the weight of the Aerial Corpuscles  
that

that lean upon it. And this weight has already so constipated the neighbouring parts of the ambient Air, that he, that would perpendicularly raise the upper Marble from the lower, shall need a considerable force to make the Revulsion, and compel the already contiguous parts of the incumbent Air to a subingression into the pores or intervals intercepted between them. For the *Conatus* of him, that endeavours to remove the upper Marble, whilst the lower surface of it is fenc'd from the pressure of the Atmosphere by the Contact of the lower Marble which suffers no Air to come in between them, is not assisted by the weight or pressure of the Atmosphere, which, when the Marbles are once separated, pressing as strongly against the undermost surface of the upper Marble, as the incumbent Atmospheric Pillar does against the upper surface of the same Marble, the hand that endeavours to raise it in the free Air has no other resistance, than that small one of the Marbles own weight to surmount. A. But

A. But what say you to the Reason that Mr. *Hobbes*, and, as he thinks, all others give of the difficulty of the often mention'd Divulsion, namely,  
*Quod spatium totum inter duo illa Marbora divulsa simul cum instantia implere Aer non potest, quantumcunque celeritate fiat divulsio.*

B. I say, that, for ought I know, the Plenists may give a more plausible account of this Experiment, than Mr. *Hobbes* has here done; and therefore abstracting from the two opposite Hypotheses, I shall further say, That the genuine Cause of the Phænomenon seems to be that which I have already assign'd; and that difficulty of raising the upper stone that accompanies the Airs not being able to come in all at once, to possess the space left between the surfaces of the two Marbles upon their separation, proceeds from hence, that, 'till that space be fill'd with the Atmospherical Air, the hand of him that would lift up the superiour Marble cannot be fully assist'd by the pressure of the

the Air against the lower surface of  
that Marble.

*A.* This is a Paradox, and therefore I shall desire to know on what you ground it?

*B.* Though I mention it but as a Conjecture propos'd *ex abundanti*, yet I shall on this occasion countenance it with two things; the first, that, since I declare not for the Hypothesis of the *Plenists* as 'tis maintain'd by Mr. *Hobbes*, I am not bound to allow, what the common Explication, adopted by my Adversary, supposes; namely, that either Nature abhors a *Vacuum* (as the Schools would have it,) or that there could be no Divulsion of the Marbles, unless at the same time the Air were admitted into the room that Divulsion makes for it. And a *Vacuist* may tell you, that, provided the strength employ'd to draw up the superiour Marble be great enough to surmount the weight of the Aerial Corpuscles accumulated upon it, the divulsion would ensue, though by Divine Omnipotence no Air

Air or other Body should be permitted to fill the room made for it by the divulsion ; and that the Air's rushing into that space does not necessarily accompany, but in order of Nature and time follow upon, a separation of the Marbles, the Air that surrounded their contiguous surfaces being by the weight of the collaterally superiour Air impell'd into the room newly made by the divulsion. But I shall rather countenance what you call my Paradox by an Experiment I purposely made in our Pneumatical Receiver, where having accommodated two flat and polish'd Marbles, so that the lower being fixt, the upper might be laid upon it and drawn up again as there should be occasion, I found, that if, when the Receiver was well exhausted, the upper Marble was by a certain contrivance laid flat upon the lower, they would not then cohere as formerly, but be with great ease separated, though it did not by any Phænomenon appear, that any Air could

come to rush in, to possess the place given it by the recess of the upper Marble, whose very easie avulsion is as easily explicable by our Hypothesis; since the pressure of that little Air, that remain'd in the Receiver, being too faint to make any at all considerable resistance to the avulsion of the upper Marble, the hand that drew it up had very little more than the single weight of the stone to surmount.

A. An *Anti-plenist* had expected, that you would have observed, that the difficult separation of the Marbles in the open Air does rather prove, that there may be a *Vacuum*, than that there can be none. For in case the Air can succeed as fast at the sides as the divulsion is made, a *Vacuity* may demand, whence comes the difficulty of the separation? And if the Air cannot fill the whole room made for it by the separated Marbles at the same instant they are forc'd asunder, how is a *Vacuum* avoided for that time, how small soever, that is necessary

cessary for the Air to pass from the edges to the middle of the room newly made?

B. What the *Plenists* will say to your Argument I leave them to consider; but I presume, they will be able to give a more plausible account of the Phænomenon we are treating of, than is given by Mr. *Hobbes*.

A. What induces you to dislike his Explication of it?

B. Two things; the one, that I think the Cause he assigns improbable; and the other, that I think another, that is better, has been assign'd already.

And *first*, whereas Mr. *Hobbes* requires to the Divulsion of the Marbles a force great enough to surmount the hardness of the stone, this is asserted *gratis*, which it should not be; since it seems very unlikely, that the weight of so few pounds as will suffice to separate two coherent Marbles of about an Inch, for instance, in Diameter, should be able to surmount the hardness of such solid

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stones as we usually employ in this Experiment. And though it be generally judg'd more easie to bend, if it may be, or break a broader piece of Marble *ceteris paribus*, than a much narrower; yet, whereas neither I, nor any else that I know, nor I believe Mr. Hobbes, ever observ'd any difference in the resistance of Marbles to separation from the greater or lesser thickness of the stones; I find by constant experience, that, *ceteris paribus*, the broadness of the coherent Marbles does exceedingly increase the difficulty of disjoining them: Insomuch that, whereas not many pounds, as I was saying, would separate Marbles of an Inch, or a lesser, Diameter; when I increased their Diameter to about four Inches, if I misremember not, there were several Men that successively try'd to pull them asunder without being able by their utmost force to effect it.

A. But what say you to the Illustration, that Mr. Hobbes, upon the supposition of the Worlds Plenitude,  
..... gives

gives of our Phænomenon by drawing asunder the opposite parts of a piece of Wax?

B. To me it seems an Instance improper enough. For *first*, the parts that are to be divided in the Wax are of a soft and yielding consistence, and according to him of a ductile, or, if you please, of a tractile nature, and not, as the parts of the coherent Marbles, very solid and hard. *Next*, the parts of the Wax do not stick together barely by a superficial contact of two smooth Planes, as do the Marbles we are speaking of; but have their parts implicated, and as it were intangled with one another. And therefore they are far from a disposition to slide off, like the Marbles, from one another, in how commodious a posture soever you place them. Besides 'tis manifest, that the Air has opportunity to succeed in the places successively deserted by the receding parts of the attenuated Wax; but 'tis neither manifest, nor as yet well proved by Mr. *Hobbes*, that the

Air does after the same manner succeed between the two Marbles, which , as I lately noted, are not forced asunder after such a way, but are, as himself speaks , sever'd in all their points at the same instant.

**A.** I know, you forget not what he says of the dividing of a hard Column into two parts by a force sufficient to overcome the resistance of its hardness.

**B.** He does not here either affirm, that he , or any he can trust, has seen the thing done ; nor does he give us any such account of the way wherein the Pillar is to be broken, whether in an erected, inclined, or horizontal posture; nor describe the particular circumstances that were fit to be mention'd in order to the solution of the Phænomenon. Wherefore , 'till I be better inform'd of the matter of fact , I can scarce look upon what Mr. Hobbes says of the Pillar , as other than his Conjecture, which now I shall the rather pass by , not only because the case is differing

fering from that of our polish'd Marbles , which are actually distinct Bodies , and only contiguous in one Commissure ; but also, because I would hasten to the *second* reason of my dislike of Mr. *Hobbes*'s Explication of our Phænomenon , which is , that a better has been given already , from the pressure of the Atmosphere upon all the superficial parts of the upper Marble save those that touch the Plane of the lower .

A. You would have put fair for convincing Mr. *Hobbes* himself , at least would have put him to unusual shifts , if you had succeeded in the attempt you made , among other of your Physico-Mechanical Experiments , to disjoyn two coherent Marbles , by suspending them horizontally in your Pneumatical Receiver , and pumping out the Air that inviron'd them ; for , from your failing in that attempt , though you render'd a not improbable Reason of it , Mr. *Hobbes* took occasion , in his Dialogue *De Natura Aeris* , to speak in

so high a strain as this: *Nihil iſthic erat quod ageret pondus; Experimento hoc excogitari contra opinionem eorum qui Vacuum aſſerunt aliud argumentum fortius aut evidentius non potuit.* Nam si duorum cohærentium alterutrum secundūm eam viam, in qua jacent ipſae contiguae superficies, propulſum eſſet, facile ſepararentur, Aere praximo in locum relictum ſuccellivē ſemper influente; ſed illa ita diuellere, ut ſimul totum amitterent contactum, imposſibile eſt, mundo pleno. Oporteret enim aut motum fieri ab uno termino ad alium in instante, aut duo corpora eodem tempore in eodem eſſe loco: *Quorum utrumvis dicere, eſt absurdum.*

B. You may remember, that where I relate that Experiment, I express'd a hope, that, when I ſhould be better accommodated than I then was, I might attempt the Tryal with prosperous ſuccell, and accordingly afterwards, having got a leſſer Engine than that I uſed before, wherewith the Air might be better pumpt out and longer kept out, I cheerfully repeated the Tryal. To shew then, that

that when two coherent Marbles are sustained horizontally in the Air, the Cause, why they are not to be forc'd asunder, if they have two or three Inches in Diameter, without the help of a considerable weight, is the pressure I was lately mentioning of the ambient Air; I caused two such coherent Marbles to be suspended in a large Receiver, with a weight at the lowermost, that might help to keep them stiddy, but was very inconsiderable to that which their Cohesion might have surmounted; then causing the Air to be pump't by degrees out of the Receiver, for a good while the Marbles stuck close together, because during that time the Air could not be so far pump't out, but that there remained enough to sustain the small weight that endeavoured their divulsion: But when the Air was further pump't out, at length the Spring of the little, but not a little expanded, Air, that remained, being grown too weak to sustain the lower Marble and

and its small clog, they did, as I expected, drop off.

A. This will not agree over-well with the confident and triumphant expressions just now recited.

B. I never envied Mr. Hobbes's forwardness to triumph, and am content, his Conjectures be recommended by the confidence that accompanies them, if mine be by the success that follows them. But to confirm the Explication given by me of our Phænomena, I shall add, that as the last mention'd Tryal, which I had several times occasion to repeat, shews, that the cohesion of our two contiguous Marbles would cease upon the withdrawing of the pressure of the Atmosphere; so by another Experiment I made, it appears, that the supervening of that pressure sufficed to cause that Cohesion. For, in prosecution of one of the lately mentioned Tryals, having found, that when the Receiver was well exhausted, two Marbles, though considerably broad, being laid upon one

ano-

another after the requisite manner, their adhesion was, if any at all, so weak, that the uppermost would be easily drawn up from off the other; we laid them again one upon the other, and then letting the external Air flow into the Receiver, we found, according to expectation, that the Marbles now cohered well, and we could not raise the uppermost but accompanied with the lowermost. But I am sensible, I have detained you too long upon the single Experiment of the Marbles: And though I hope the stress Mr. Hobbes lays on it will plead my excuse, yet to make your Patience some amends, I shall be the more brief in the other particulars that remain to be consider'd in his Dialogue *De Vacuo*. And 'twill not be difficult for me to keep my promise without injuring my Cause, since almost all these particulars being but the same which he has already alledged in his Dialogue *De Natura Aeris*, and I soon after answered in my *Examen* of that Dialogue,

logue, I shall need but to refer you to the passages where you may find these Allegations examin'd, only ſubjoyning here ſome Reflections upon those few and flight things, that he has added in his Problems *De Vacuo*.

A. I may then, I suppose, read to you the next paſſage to that long one, you have hitherto been conſidering, and it is this: *Ad Vacuum nunc revertor: Quas causas fine ſuppoſitione Vacui redditurus es illorum effectum, qui ostenduntur per Machinam illam que eſt in Collegio Greshamensi?*

B. *Machina illa —*

B. Stop here, I beſeech you, a little, that, before we go any further, I may take notice to you of a couple of things that will concern our ſubſequent Discourefe.

Whereof the firſt is, that it appears by Mr. Hobbes's *Dialogue about the Air*, that the Explications he there gave of ſome of the Phænomena of the *Machina Boyleiana*, were directed partly againſt the *Virtuosi*, that have ſince been honour'd with the Title  
of

of the Royal Society, and partly against the Author of that Engine, as if the main thing therein design'd were to prove a *Vacuum*. And since he now repeats the same explications, I think it necessary to say again, that if he either takes the *Society* or me for profess'd *Vacuists*, he mistakes, and shoots beside the mark; for, neither they nor I have ever yet declar'd either for or against a *Vacuum*.

And the other thing I would observe to you, is, that Mr. Hobbes seems not to have rightly understood, or at least not to have sufficiently heeded in what chiefly consists the advantage, which the *Vacuists* may make of our Engine against him: For, whereas in divers places he is very solicitous to prove, that the cavity of our Pneumatical Receiver is not altogether empty, the *Vacuists* may tell him, that since he asserts the *absolute* plenitude of the World, he must, as indeed he does, reject not only great Vacuities, but also those very small and interspers'd ones,

ones, that they ſuppoſe to be intercep-  
ted between the ſolid corpuscles  
of other bodies, particularly of the  
Air: So that it would not conſute  
them to prove, that in our Receiver,  
when moſt diligently exhausted,  
there is not one great and abſolute  
Vacuity, or, as they ſpeak, a *Vacuum  
conſervatum*, ſince ſmaller and diſſe-  
minated Vacuitieſ would ſerve their  
turn. And therefore they may think  
their Pretenſions highly favourable, as  
by feveſal particular effects, ſo by  
this general Phænomenon of our En-  
gine, that it appears by feveſal Cir-  
cumſtances, that the Common or At-  
moſpherical Air, which, before  
the pump is ſet a work, poſſeſd  
the whole cavity of our Receiver,  
far the greateſt part is by the inter-  
vention of the pump made to paſſ  
out of the cavity into the open Air,  
without being able, at leaſt for a  
little while, to get in again; and  
yet it does not appear by any thing  
alleg'd by Mr. Hobbes, that any o-  
ther body ſucceeds to fill adequately  
the

the places deserted by such a multitude of Aerial corpuscles.

A. If I gheſſ aright, by those words, (viz. it appears not by any thing alledged by Mr. Hobbes,) you design to intimate, that you would not in general prejudice the Plenists.

B. Your conjecture was well founded: For I think divers of them, and particularly the Cartesians, who suppose a subtle Matter or *Aether* fine enough to permeate glass, though our common Air cannot do it, have not near so difficult a task to avoid the Arguments the Vacuists may draw from our Engine, as Mr. Hobbes, who, without having recourse to the porosity of glafs, which indeed is impervious to *common* Air, strives to solve the Phænomena, and prove our Receiver to be always perfectly full, and therefore as full at any one time as at any other of common or Atmospheric Air, as far as we can judge of his opinion by the tendency or import of his Explications.

A. Yet, if I were rightly inform'd  
of

of an Experiment of yours, Mr. Hobbes may be thereby reduc'd either to pass over to the *Vacuists*, or to acknowledge some Aëtherial or other matter more subtil than Air, and capable of passing through the pores of glas; and therefore, to shew your self impartial between the Vacuists and their Adversaries in this Controversie, I hope you will not refuse to gratifie the *Plenists* by giving your friends a more particular account of the Experiment.

B. I know which you mean, and remember it very well. For, though I long since devis'd it, yet having but the other day had occasion to peruse the Relation I writ down of one of the best Tryals, I think I can repeat it, almost in the very words, which, if I mistake not, were these:

There was taken a Bubble of thin white glas, about the bigness of a Nutmeg, with a very slender stem, of about four or five Inches long, and of the bigness of a Crows-quill.  
The

The end of the Quill being held in the flame of a Lamp blown with a pair of Bellows, was readily and well seal'd up, and presently the globous part of the glass, being held by the stem, was kept turning in the flame, 'till it was red hot and ready to melt; then being a little removed from the flame, as the included Air began to lose of its agitation and spring, the external Air manifestly and considerably press'd in one of the sides of the Bubble. But the glass being again, before the cold could crack it, held as before in the flame, the rarified Air distended and plump'd up the Bubble; which being the second time remov'd from the flame, was the second time compress'd; and, being the third time brought back to the flame, swell'd as before, and remov'd, was again compress'd, (either this time or the last by two distinct cavities;) 'till at length, having satisfied our selves, that the included Air was capable of being condens'd or dilated without the ingreſs or

egress of Air (properly so called) we held the Bubble so long in the flame, strengthen'd by nimble blasts, that not only it had its sides plump'd up, but a hole violently broken in it by the over-rarified Air, which, together with the former watchfulness, we employ'd from time to time to discern if it were any where crackt or perforated, satisfied us that it was till then intire.

A. I confess, I did not readily conceive before, how you could, (as I was told you had,) make a solid Vessel, wherein there was no danger of the Aires getting in or out, whose cavity should be still possest with the same Air, and yet the Vessel be made by turns bigger and lesser. And, though I presently thought upon a well stopt bladder, yet I well foresaw, that a distrustful Adversary might make some Objections, which are by your way of proceeding obviated, and the Experiment agrees with your Doctrine in shewing, how impervious we may well think your thick

Pneu-

Pneumatick Receivers are to common Air, since a thin glass Bubble, when its pores were open'd or relax'd by flame, would not give paſſage to the Springy particles of the Air, though violently agitated; for if those particles could have got out of the pores, they never would have broke the Bubble, as at length a more violent degree of Heat made them do; nor probably would the Compression, that afterwards infued of the Bubble by the ambient Air, be checkt near so ſoon, if those Springy Corpuscles had not remained within to make the reſiſtance. Methinks, one may hence draw a new proof of what I remember you elsewhere teach, that the Spring of the Air may be much strengthen'd by Heat. For, in our caſe, the Spring of the Air was thereby inabled to expand the compreſt glaſs, it was impriſon'd in, in ſpite of the reſiſting pressure of the external Air; and yet, that this pressure was conſiderable, appears by this, that the weight of ſo ſmall a Column

of Atmospheric Air, as could bear upon the Bubble, was able to press in the heated glass, in spite of the resistance of its tenacity and arched figure.

B. Yet that which I mainly design'd in this Experiment was, (if I were able) to shew and prove at once, by an Instance not lyable to the ordinary exceptions, the true Nature of Rarefaction and Condensation, at least of the Air. For, to say nothing of the *Peripatetick* Rarefaction and Condensation, strictly so call'd, which I scruple not to declare, I think to be physically inconceivable or impossible; 'tis plain by our Experiment, that, when the Bubble, after the Glass had been first thrust in towards the Center, was expanded again by heat, the included Air *possess'd* more room than before, and yet it could perfectly fill no more room than formerly, each Aerial Particle taking up, both before and after the heating of the Bubble, a portion of space adequate to its own bulk; so that in the Cavity of the expan-

expanded Bubble we must admit either Vacuities interspers'd between the Corpuscles of the Air, or that some fine Particles of the Flame, or other subtil matter, came in to fill up those Intervals, which matter must have enter'd the Cavity of the Glass at its pores: And afterwards, when the red-hot Bubble was removed from the flame, it is evident, that, since the grosser particles of the Air could not get through the Glass, which they were not able to do, even when vehemently agitated by an ambient Flame, the Compression of the Bubble, and the Condensation of the Air, which was necessarily consequent upon it, could not, supposing the Plenitude of the World, be performed without squeezing out some of the subtil matter contained in the cavity of the Bubble, whence it could not issue but at the pores of the Glass. But I will no longer detain you from Mr. Hobbes his Explications of the *Machina Boyleiana*; to the first of which you may now, if you please, advance.

A. The passage I was going to read, when you interrupted me, was this :

B. *Machina illa eosdem effectus producit, quos produceret in loco non magno magnus inclusus ventus.*

A. *Quomodo ingreditur istuc ventus? Machinam nosti Cylindrum esse cavum, aneum, in quem protruditur Cylindrus alius solidus ligneus, corio tectus, (quem suctorum dicunt) ita exquisitè congruens, ut ne minimus quidem Aer inter corium & eis intrare (ut putant) possit.*

B. *Scio, & quò suctor facilius intrudi possit, foramen quoddam est in superiori parte Cylindri, per quod Aer (qui suctoris ingressum alioqui impedire possit) emittatur. Quod foramen aperire possunt & claudere quoties usus postulat. Est etiam in Cylindri cavi recessu summo datus aditus Aeri in globum concavum Vitreum, quem etiam aditum clavicula obturare & aperire possunt quoties volunt. Denique in globo vitreo summo relinquuntur foramen satis amplum, (clavicula item claudendum & recludendum) ut in illum quæ volunt immittere possint, experiendi causa.*

B. The

B. The imaginary wind to which Mr. *Hobbes* here ascribes the effects of our Engine, he formerly had recourse to in the 13<sup>th</sup> page of his *Dialogue*, and I have sufficiently answer'd that passage of it in the 45<sup>th</sup> and 46<sup>th</sup> pages of my *Examen*, to which I therefore refer you.

A. I presume, you did not overlook the comparison Mr. *Hobbes* annexes to what I last read out of his Problems, since he liked the conceit so well, that we meet with it in this place again, though he had formerly printed it in his Dialogue *De Natura Aeris*. The words (as you see) are these: *Tota denique Machina non multum differt, si naturam ejus spectes, à Sclopeto ex Sambuco, quo pueri se delectant, imitantes Sclopetos militum, nisi quod major sit, & majori arte fabricatus, & pluris constet.*

B. I could scarce, for the reason you give, avoid taking notice of it. And if Mr. *Hobbes* intended it for a piece of Ralliery, I willingly let it pass, and could easily forgive him a more

considerable attempt than this, to be reveng'd on an Engine that has destroyed several of his opinions : But, if he seriously meant to make a Physical Comparison, I think he made a very improper one. For, not to urge, that one may well doubt how he knows, that in the inclosed cavity of his Pot-gun, there is a very vehement wind, (since that does not necessarily follow from the compression of the included Air:) In Mr. Hobbes's Instrument, the Air, being forcibly comprehended, has an endeavour to expand it self, and when it is able to surmount the resistance of its prison, that part that is first disjoyn'd is forcibly thrown outwards; whereas in our Engine it appears by the passage lately cited of our *Examen*, that the Air is not comprehended but expanded in our Receiver, and if an intercourse be open'd, or the Vessel be not strong enough, the outward Air violently rushes in: And if the Receiver chance to break, the fragments of the glass are not thrown outwards, but forced inwards.

A. So

A. So that, whether or no Mr. *Hobbes* could have pitch'd upon a Comparison more suitable to his Intentions, he might easily have employed one more suitable to the *Phænomena*.

B. I presume, you will judge it the less agreeable to the Phænomena, if I here subjoyn an Experiment, that possibly you will not dislike; which I devis'd to shew, not only that in our exhausted Receivers there is no such strong endeavour outwards, as most of Mr. *Hobbes*'s Explications of the things that happen in them are built upon, but that the weight of the Atmospheric Air, when 'tis not resisted by the counterpressure of any internal Air, is able to perform what a weight of many pounds would not suffice to do.

A. I shall the more willingly learn an Experiment to this purpose, because in your Receivers, the rigidity of the glass keeps us from seeing, by any manifest change of its figure, whether, if it could yield without breaking,

breaking, it would be press'd in, as your Hypothesis requires.

B. The desires to obviate that very difficulty, for their satisfaction, that had not yet penetrated the grounds of our *Hypothesis*, made me think of employing, instead of a Receiver of Glass, one of a stiff and tough, but yet somewhat flexible, Metal. And accordingly having provided a new Pewter Porrenger, and whelm'd it upside down upon an Iron plate fasten'd to (the upper end of) our Pneumatical Pump, we carefully fasten'd by Cement the orifice to the plate, and though the inverted Vessel, by reason of its stiffness and thickness and the convexity of its superficies, were strong enough to have supported a great weight without changing its figure; yet, as soon as by an exsuction or two the remaining part of the included Air was brought to such a degree of expansion, that its weaken'd Spring was able to afford but little assistance to the tenacity and firmness of the Metal,

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the weight of the pillar of the incumbent Atmosphere (which by reason of the breadth of the Vessel was considerably wide also) did presently and notably depress the upper part of the Porringer, both lessening its capacity and changing its figure; so that instead of the Convex surface, the Receiver had before, it came to a Concave one, which new figure was somewhat, though not much, increased by the further withdrawing of the included and already rarified Air. The Experiment succeeded also with an other common Porringer of the same Metal. But in such kind of Vessels, made purposely of Iron plates, it will sometimes succeed and sometimes not, according to the Diameter of the vessel and the thickness of the plate, which was sometimes strong enough and sometimes too weak to resist the pressure of the incumbent Air. And sometimes I found also, that the vessel would be thrust in, not at the top but side-ways, in case that side were the only part that were

were made too thin to resist the pressure of the Ambient; which Phænomenon I therefore take notice of, that you may see, that that powerful pressure may be exercised laterally as well as perpendicularly.

Perhaps this Experiment, and that I lately recited of an Hermetically sealed Bubble, by their fitness to disprove Mr. Hobbes's Doctrine, may do somewhat towards the letting him see, that he might have spar'd that not over-modest and wary expression, where speaking of the Gentlemen that meet at Gresham-College, (of whom I pretend not to be one of the chief) he is pleased to say, *Experimenta faciant quantum volunt, nisi Principiis utantur meis nihil proficient.* But let us, if you please, pass on to what he further alledges to prove, that the space in the exhausted Receiver, which the Vacuists suppose to be partly empty, is full of Air. (*Video* (says A.) *si suctor trudatur usque ad fundum Cylindri Aenei, obturenturque foramina, Secuturum esse, dum suctor retrahitur, locum*

locum in Cylindro cavo relictum fore vacuum. Nam ut in locum ejus succedat Aer, est impossibile. To which B. answers, *Credo equidem, suuctorem cum Cylindri cavi superficie satis arcte cohaerere ad excludendum stramen & plumam, non autem Aerem neque Aquam.* Cogita enim, quod non ita accuratè congruerent, quin undiquaque interstitium relinquoretur, quantum tenuissimi capilli capax esset. Retracto ergo suuctore, tantum impelleretur Aeris, quantum viribus illis conveniret quibus Aer propter suuctoris Retractionem reprimitur, idque sine omni difficultate sensibili. *Quanto autem interstitium illud minus esset, tantum ingredieretur Aer velocius: Vel si contactus sit, sed non per omnia puncta, etiam tunc intrabit Aer, modo suuctor maiore vi retrahatur.* Postremò, et si contactus ubique exactissimus sit, vi tamen satis aucta per cochleam ferreane, tum corium cedet, tum ipsum es; atque ita quoque ingredietur Aer. Credin' tu, possibile esse duas superficies ita exacte componere, ut has compositas esse supponunt illi; aut corium ita durum esse,

*ut Aeri, qui Cochlear ope incutitur, nihil omnino cedat? Corium quanquam optimum admittit aquam, ut ipse scis, si forte fecisti unquam iter vento & pluvia oculum tuum arietem. Itaque dubitare non potes, quin retractus Suctior tantum Aeris in Cylindrum adeoque in ipsum Recipientis incutiat, quantum sufficit ad locum semper relictum perfectè implendum.* Effectus ergo, qui oritur à Retractione suctoris, alius non est quam ventus, ventus (*inquam*) vehementissimus, qui ingreditur undiquaque inter Suctoris superficiem convexam, & Cylindri aenei concavam, proceditque (*versa clavicula*) in cavitatem globi Vitrei, sive (*ut vocatur*) Recipientis.

The Substance of this Ratiocination having been already propos'd by Mr. Hobbes in his Dialogue of the Air, the 11<sup>th</sup> page, I long since answer'd it in the 30<sup>th</sup> and some of the following pages of my *Examen*; and therefore I shall only now take notice *in transitu* of some slight whether additions or variations, that occur in what you have been reading. And,

first,

first, I see no probability in what he *gratis* asserts, that so thick a Cylinder of Brass, as made the chief part of the pump of our Engine, should yield to the Sucker, that was mov'd up and down in it, though by the help of an Iron rack; and whereas he adds, that the leather, that surrounds the more solid part of the Sucker, would yield to such a force; it seems, that that compression of the leather should by thrusting the solid parts into the pores make the leather rather less than more fit to give passage to the Air; nor would it however follow, notwithstanding Mr. *Hobbes's* Example, that, because a Body admits Water, it must be pervious to Air: For I have several times, by ways elsewhere taught, made Water penetrate the pores of Bladders, and yet Bladders resist the passage of the Air so well, that even when Air included in them was sufficiently rarefied by Heat, or by our Engine, it was necessary for the Air to break them before it could get out; which would

would not have been, if it could have escap'd through their pores. What Mr. Hobbes inculcates here again concerning his *ventus vehementissimus*, you will find answer'd in the place of my *Examen I* lately directed you to.

A. We may then proceed to Mr. Hobbes's next Explication, which he proposes in these terms:

A. *Causam video nunc unius ex Machinae mirabilibus, nimirum cur Suctio, postquam est aliquatenus retractus & deinde amissus, subito recurrit ad Cylindri summitatem. Nam Aer, qui vi magna fuit impulsus, rursus per repercussionem ad externa vi eadem revertitur.*

B. *Atque hoc quidem Argumenti satis est etiam solum, quod locus à suctore relictus non est Vacuus. Quid enim aut attrahere aut impellere suctorem potuit ad locum illum unde retractus erat, si Cylindrus fuisset vacuus? Nam ut Aeris pondus aliquod id efficere potuisset, falsum esse satis supra demonstravi ab eo quod Aer in Aere gravitare non potest. Nosti etiam, quod cum ē recipiente Aerem*

Aerem omnem (ut illi loquuntur) exegere-  
rint, possunt tamen trans vitrum id  
quod intus sit videre, & sonum, si quis  
sit, inde audire. Id quod solum, et si  
nullum aliud Argumentum esset (sunt  
autem multa,) ad probandum, nullum  
esse in Recipiente Vacuum, abunde sufficit.

B. Here are several things joyn'd together, which the Author had before separately alledg'd in his often-mention'd Dialogue. The first is, the Cause he assigns of the ascension of the Sucker forcibly deprest to the bottom of the exhausted Cylinder, and then let alone by him that pumpt; to which might be added, that this ascension succeeded, when the Sucker was clogg'd with an hundred pound weight. This Explication of Mr. Hobbes you will find examin'd in the 33<sup>th</sup> and 39<sup>th</sup>, and some ensuing pages of my Discourse. And as to his denying, that the weight or pressure of the Air could drive up the Sucker in that Phænomenon, because the Air does not weigh in Air, we may see the contrary largely proved in divers

D places

places of my *Examen*, and more particularly and expressly in the four first pages of the *third Chapter*. And whereas he says in the last place, that the visibility of Bodies included in our Receivers, and the propagation of Sound, (which, by the way, is not to be understood of all Sound that may be heard, though made in the exhausted Receiver,) are alone sufficient Arguments to prove no *Vacuum*: I have consider'd that passage in the answer I made to the like allegation in the 45<sup>th</sup> page of the *Examen*; and shall only observe here, that, since the *Vacuists* can prove, that much of the Air is pump't out of the exhausted Receiver, and will pretend, that, notwithstanding many interspers'd Vacuities, there may be in the Receiver corporeal substance enough to transmit Light and stronger Sounds, Mr. *Hobbes* has not perform'd what he pretended, if he have but barely proved, that there may be Substances capable of conveying Light and Sound in the cavity of our Re-

Receiver, since he triumphantly asserts, *Nullum esse in Recipienti Vacuum.* But we may leave Mr. Hobbes and his Adversaries to dispute out this point, and go on to the next passage.

A. Which follows in these words:

*Ad illud autem, quod si Vesica aliquatenus inflata in Recipiente includatur, paulo post per exuctionem aeris inflatur vehementius & dirumpitur, quid respondes?*

B. Motus partium Aeris undiquaque concurrentium velocissimus & per concursum in spatiis brevissimis numeroque infinitis gyrationis velocissimæ vesicam in locis innumerabilibus simul & vi magna, instar totidem terebrarum, penetrat, præsertim si vesica, antequam immittatur, quò magis resistat aliquatenus inflata sit. Postquam autem Aer penetrans semel ingressus est, facile cogitare potes, quo pacto deinceps vesicam tendet, & tandem rumpet. Verùm si antequam rumpatur, versâ claviculâ, Aer externus admittatur, videbis vesicam propter vehementiam motus temperatam diminutâ tensione rugosiorē. Nam id quoque observatum est. Nam si hac, quam dixi,

*causa minus tibi videatur verisimilis, vide an tu aut alius quicunque imaginari potest, quo pacto vesica distendi & rumpi possit à viribus Vacui, id est, Nihili.*

B. This Explication Mr. Hobbes gave us in the 19<sup>th</sup> page of his Dialogue *De Natura Aeris*, and you may find it at large confuted in the latter part of the third Chapter of my *Examens*. Nor does, what he here says in the close about the *Vires Vacui* or *Nihili*, deserve to detain us, since there is no reason at all, that the *Vacuists* should ascribe to *nothing* a power of breaking a Bladder, of whose rupture the Spring of the included Air supplies them so easily with a sufficient Cause.

After what Mr. Hobbes has said of the breaking of a Bladder, he proceeds to an Experiment which he judges of affinity with it, and his Academician having propos'd this Question :

*Unde fit ut animalia tam cito, nimirum spatio quatuor minutorum hore, in recipiente interficiantur?*

For

For answer to it our Author says :

B. Nonne animalia sic inclusa insugunt in Pulmones Aerem vehementissime motum? Quo motu necesse est ut transitus sanguinis ab uno ad alterum cordis ventriculum interceptus, non multò post sistatur. Cessatio autem sanguinis, Mors est. Possunt tamen animalia cessante sanguine reviviscere, si Aer externus satis mature intromittatur, vel ipsa in Aerem temperatum, antequam refrixerit sanguis, extrahantur.

This Explication is not probable enough, to oblige me to add any thing about it to what I have said in the 49<sup>th</sup> and the two following pages of my *Examen*; especially the most vehement motion, ascrib'd to the Air in the Receiver, having been before proved to be an Imaginary thing. You may therefore, if you please, take notice of the next Explication.

[*Idem Aer* (says he) *in Recipiente Carbones ardentes extinguit, sed & illi, si, dum satis calidi sunt, eximantur, relucebunt. Notissimum est, quod in fodi- nis Carbonum terreorum (cujus rei ex-*

D 3 perimentum

perimentum ipſe vidi) ſepiſſime ē lateribus fovea& ventus quidam undiquaque exit, qui foſſores interficit ignemque extinguit, qui tamen revivifcent ſi ſatis cito ad Aerem liberum extrahan-tur.]

This Comparison which Mr. Hobbes here ſummarily makes, he more fully display'd in his Dialogue *De Natura Aeris*, and I conſider'd, what he there alledg'd, in the 52<sup>th</sup> page and the two next of my *Examen*. And, though I will not contradict Mr. Hobbes in what he historically asserts in this paſſage; yet I cannot but ſomewhat doubt, whether he mingles not his conjecture with the bare matter of fact. For, though I have with ſome curioſity viſited Mines in more places than one, and propos'd Questions to Men that have been converfant in other Mines, both elsewhere and in *England* (and particularly in *Derbyſhire* where Mr. Hobbes lived long;) yet I could never find, that any ſuch odd and vehement wind, as Mr. Hobbes ascribes the Phænomenon to, had been

been by them observed to kill the Diggers, and extinguish well-lighted Coals themselves: And indeed, it seems more likely, that the damp, by its tenacity or some peculiarly malign quality, did the mischief, than a wind, of which I found not any notice taken; especially since we see, what vehement winds Men will be able to endure for a long time, without being near-kill'd by them; and that it seems very odd, that a wind, that Mr. *Hobbes* does not observe to have blown away the Coals, that were let down, should be able (instead of kindling them more fiercely) to blow them out.

A. The last Experiment of your Engine, that your Adversary mentions in these Problems, is deliver'd in this passage:

A. *Si phialam aquæ in Recipientis dimiseris, exucto Aere bullire videbis aquam. Quid ad hoc Respondebis?*

B. *Credo sanè in tanta Aeris motitatione saltaturam esse aquam, sed ut calefiat nondum audivi. Sed imagina-*

D 4                      bile

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*bile non eſt*, Saltationem illam à Vacuo  
nasci poſſe.

B. This Phænomenon he likewife took notice of, and attempted to explicate in his above-mention'd Dialogue, which gave me occasion in the 46<sup>th</sup> and 47<sup>th</sup> pages of my *Examen*, to shew how unlikely 'tis, that the vehement motion of the Air should be the cause of it; but he here tells us, that 'tis not imaginable, that this dancing of the water (as he is pleas'd to call it) proceeds from a *vacuum*, nor do I know any Man that ever pretended, that a *vacuum* was the efficient cause of it. But the *Vacuifts* perhaps will tell him, that, though the bubbling of the water be not an *effect* of a *vacuum*, it may be a *proof* of it against him; for they will tell him, that it has been formerly proved, that a great part of the Atmospherical Air is by pumping remov'd out of our exhausted Receiver, and consequently can no more, as formerly, presſ upon the surface of the water. Nor does Mr. Hobbes shew

shew what succeeds in the room of it ; and therefore it will be allowable, for them to conclude against him (though not perhaps against the *Cartesians*) that there are a great many interspers'd Vacuities left in the Receiver, which are the occasion, though not the proper efficient cause, of the Phænomenon. For they will say , that the Springy Particles of the yet included Air, having room to unbend themselves in the spaces deserted by the Air that was pump't out, the Aerial and Springy Corpuscles, that lay conceal'd in the pores of the water, being now freed from the wonted pressure that kept them coil'd up in the liquor , expanded themselves into numerous bubbles , which , because of their comparative lightness, are extruded by the water , and many of them appear to have risen from the bottom of it. And Mr. *Hobbes*'s vehement wind , to produce the several Circumstances of this Experiment , must be a lasting one. For , after the agitation of the Pump has been

been quite left off, provided the external Air be kept from getting in, the bubbles will sometimes continueto rise for an hour after. And that which agrees very well with our Explication and very ill with that of Mr Hobbes's, is , that, when by having continued to pump a competent time, the wa-  
ter has been freed from the Aerial particles that lurk'd in it before, though one continue to pump as lu-  
stily as he did, yet the water will not at all be cover'd with bubbles as it was, the Air that produc'd them being spent ; though , according to Mr. Hobbes's Explication , the wind in the Receiver continuing, the dance of the water should continue too.

A. I easily ghesse , by what you have said already , what you may say of that *Epiphonema* wherewith Mr. Hobbes (in his 18<sup>th</sup> page) concludes the Explications of the Phænomena of your Engine. [ *Spero jam te cer-  
tum esse*, says he, *nullum esse Machine illius Phænomenon, quo demonstrari potest  
ullum in Universo locum dari corpore omni  
vacuum.* ]

B. If

B. If you gheſſ'd aright, you gheſſ'd that I would ſay, that as to the Phænomena of my Engine, my buſineſſ was to prove, that he had not ſubſtituted good Explications of them in the place of mine, which he was pleaſed to rejeſt. And as for the proving a *Vacuum* by the Phænomena of my Engine, though I declar'd that was not the thing intended, yet I ſhall not wonder, that the *Vacuifts* ſhould think those Phænomena give them an advantage againſt Mr. *Hobbes*. For, though in the paſſage recited by you he ſpeak more cauſiouſly than he is wont to do, yet, by what you may have alreaſdy obſerv'd in his Argumentations, the way he takes to ſolve the Phænomena of our Engine, is by con-tending, that our Receiver, when we ſay it is almoſt exhausted, is as full as ever (for he will have it *per-fectly* full,) of common Air; which is a conceit ſo contrary to I know not how many Phænomena, that I do not remember I have met with or heard

heard of any Naturalist, whether Vacuist or Plenist, that having read my Physico-Mechanical Experiments and his Dialogue, has embrac'd his opinion.

A. After what you have said, I will not trouble you with what he subjoyns about *Vacuum* in general, where having made his Academian say, [*Mundum scis finitum esse, & per Consequens vacuum esse oportere totum illud Spatium quod est extra mundum infinitum. Quid impedit quo minus vacuum illud cum Aere mundano permisceatur?*] He answers: *De rebus transmundanis nihil scio.* For I know, that it concerns not you to take notice of it. But possibly the *Vacuists* will think, he fathers upon them an Impropriety they would not be guilty of, making them speak, as if they thought, the *ultra-mundan Vacuum* were a real Substance that might be brought into this World and mingled with our Air. And since, for ought I know, Mr. *Hobbes* might have spar'd this passage, if he had not design'd

sign'd it should introduce the flighting answer he makes to it; I shall add, that by the account Mr. *Hobbes* has given of several Phænomena *within the World*, 'tis possible, that the Vacuists may believe his Profession of knowing nothing of things beyond it.

After the *Experimenta Boyliana* (as your other Adversary calls them;) Mr. *Hobbes* proceeds to the Torricelian Experiment, of which he thus discourses:

A. *Quid de experimento censes Torricelliano, probante Vacuum per Argentum vivum hoc modo: est in seq. figura ad A, pelvis sive aliud vas, & in eo Argentum vivum usque ad B; est autem C D tubus vitreus concavus repletus quoque Argento vivo. Hunc tubum si digito obturaveris erexerisque in vase A, manumque abstuleris, descendet Argentum vivum à C; verum non effundetur totum in pelvim, sed sistetur in distantia quadam, puta in D. Nonne ergo necessarium est, ut pars tubi inter C & D sit vacua? Non enim puto negabis quin*

*quin superficies tubi concava & Argenti  
vivi convexa se mutuo exquisitissime  
contingant.*

B. *Ego neque nego contactum, neque  
vim Conſequentia intelligo.*

By which paſſage it ſeems that he ſtill perſiſts in the ſolution of this Experiment, which he gave in his Dialogue *De Natura Aeris*, and formerly did, for the main, either propoſe, or adopt, in his Elements of Philoſophy.

B. This opinion or explication of Mr. Hobbes I have, as far as concerns me, conſider'd in the 36<sup>th</sup>, and ſome inſuing pages, of my *Examen*, to which it may well ſuffice me to refer you. But yet let me take notice of what he now alledges:

B. *Si quis (says he) in Argentum  
vivum, quod in vase eſt, veficam im-  
merserit inflatam, nonne illa amotā manu  
emerget?*

A. *Ita certè, etſi eſſet vefica ferrea  
vel ex materia quacunque preter Aurum.*

B. *Vides igitur ab Aere penetrari  
poſſe Argentum vivum.*

A. *Etiā,*

A. *Etiam, & quidem illâ ipsâ vi quam à pondere accipit Argenti vivi.*

I confess this Allegation did a little surprize me: It concern'd Mr. *Hobbes* to prove, that as much Air, as was displac'd by the descending *Mércury*, did at the orifice of the Tube, immers'd in stagnant *Mercury*, invisibly ascend to the upper part of the pipe. To prove this he tells us, that a bladder full of Air being depresso'd in *Quicksilver*, will, when the hand that depresso'd it is remov'd, be squeez'd up by the very weight of the *Mercury*, whence it follows, that Air may penetrate *Quicksilver*. But I know not, who ever deny'd, that Air environ'd with *Quicksilver* may thereby be squeez'd upwards; but, since even very small bubbles of Air may be seen to move in their passage through *Mercury*, I see not, how this Example will at all help the Proposer of it. For 'tis by meer accident, that the Air included in the bladder comes to be buoy'd up, because the bladder it self is so; and if it

it were fill'd with Water instead of Air, or with Stone instead of Water, it would nevertheless emerge, as himself confesses it would do, if it were made of Iron, or of any Matter besides Gold, because all other Bodies are lighter *in specie* than Quicksilver. But since the emersion of the bladder is manifest enough to the sight, I see not how it will serve Mr. Hobbes's turn, who is to prove that the Air gets into the Torricelian Tube invisibly; since 'tis plain, that even heedful observation can make our Eyes discover no such trajectio[n] of the Air; which (to add that inforcement of our Argument) must not only pass unseen through the sustained Quicksilver, but must likewise unperceivedly dive, in spite of its comparative lightness, beneath the surface of the ponderous stagnant Mercury, to get in at the orifice of the erected Tube. But let us, if you please, hear the rest of his Discourse about this Experiment.

A. Though it be somewhat prolix,

lix, yet, according to my custom hitherto, I will give it you verbatim.

B. *Simul atque Argentum vivum descendenterit ad D, altius erit in vase A quam antè, nimisrum plus erit Argenti vivi in vase quam erat ante descensum, tanto quantum capit pars tubi C, D. Tanto quoque minus erit Aeris extra tubum quam ante erat. Ille autem Aer qui ab Argento vivo loco suo extrusus est, (supposita universi plenitudine) quā abire potest nisi ad eum locum, qui in tubo inter C & D à descensu Argenti vivi relinquebatur? sed quā, inquires, viā in illum locum successurus est? Quā, nisi per ipsum corpus Argenti vivi Aerem urgenter? Sicut enim omne grave liquidum, sui ipsius pondere, Aerem, quem descendendo premit, ascendere cogit (si via alia non detur) per suum ipsius corpus; ita quoque Aerem quem premit ascendendo, (si via alia non detur) per suum ipsius corpus transire cogit. Manifestum igitur est, supposita mundi plenitudine posse Aerem externum ab ipsa gravitate Argenti vivi cogi in locum illum inter C & D. Itaque Phenomenon illud necessitatē vacui non*

E demon-

demonstrat. Quoniam autem corpus Argenti vivi penetrationi, quæ fit ab Aere, non nihil resistit, & ascensioni Argenti vivi in vase A resistit Aer; quando ille due resistantie æquales erunt, tunc in tubo sistetur alicubi Argentum vivum; atque ibi est D.

*51 114 K. 1.*

B. In answer to this Explication I have in my *Examen* propos'd divers things, which you may there meet with: And indeed his Explication has appear'd so improbable to those that have written of this Experiment, that I have not found it embrac'd by any of them, though, when divers of them oppos'd it, the Phænomena of our Engine were not yet divulg'd. Not then needlessly to repeat what has been said already, I shall on this occasion only add one Experiment, that I afterwards made, and it was this: Having made the Torricellian Experiment (in a straight Tube) after the ordinary way, we took a little piece of a fine Bladder, and raising the Pipe a little in the stagnant Mercury, but not so high as

as the surface of it, the piece of Bladder was dexterously conveyed in the Quicksilver, so as to be applied by ones finger to the immersed orifice of the Pipe, without letting the Air get into the Cavity of it; then the Bladder was tyed very straight and carefully to the lower end of the Pipe, whose orifice (as we said) it cover'd before, and then the Pipe being slowly lifted out of the stagnat Mercury, the impendent Quicksilver appear'd to lean but very lightly upon the Bladder, being so near an exact Aequilibrium with the Atmosperical Air, that, if the Tube were but a very little inclin'd, whereby the gravitation of the Quicksilver, being not so perpendicular, came to be somewhat lessen'd, the Bladder would immediately be driven into the orifice of the Tube, and to the Eye, plac'd without, appear to have acquir'd a concave superficies instead of the convex it had before. And when the Tube was re-erected, the Bladder would no longer appear

E 2                   fuck'd

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fuck'd in ; but be again somewhat protuberant. And if , when the Mercury in the Pipe was made to descend a little below its station into the stagnant Mercury , if , I say , at that nick of time the piece of Bladder were nimbly and dexterously apply'd , as before , to the immers'd orifice , and fasten'd to the sides of the Pipe , upon the lifting the Instrument out of the stagnant Mercury , the Cylinder of that Liquor being now somewhat short of its due height , was no longer able fully to counterpoise the weight of the Atmospheric Air , which consequently , though the Glass were held in an erected posture , would press up the Bladder into the orifice of the Pipe , and both make and maintain there a Cavity sensible both to the Touch and the Eye .

A. What did you mainly drive at in this Experiment ?

B. To satisfie some Ingenious Men , that were more diffident of , than skilful in , Hydrostaticks , that the

the pressure of the external Air is capable of sustaining a Cylinder of 29 or 30 Inches of Mercury, and upon a small lessening of the gravitation of that ponderous liquor, to press it up higher into the Tube. But a farther use may be made of it against Mr. Hobbes's pretension. For, when the Tube is again erected, the Mercury will subside as low as at first, and leave as great a space as formerly was left deserted at the top; into which how the Air should get to fill it, will not appear easie to them, that, like you and me, know by many tryals, that a Bladder will rather be burst by Air than grant it passage. And if it should be pretended, either that some Air from without had yet got through the Bladder, or that the Air, that they may presume to have been just before included between the Bladder and the Mercury, made its way from the lower part of the Instrument to the upper; 'tis obvious to answer, That 'tis no way likely, that it should pass all along the Cy-

linder unseen by us ; since , when there are really any Aerial Bubbles, though smaller than Pins heads, they are easily discernible. And in our case , there is no such resistance of the Air to the ascension of the stagnant Mercury, as Mr. *Hobbes* pretends in the Torricellian Experiment made the usual way.

A. But ; whatever becomes of Mr. *Hobbes's* Explication of the Phænomenon ; yet may not one still say, that it affords no advantage to the *Vacuists* against him ?

B. Whether or no it do against other *Plenists* , I shall not now consider ; but I doubt, the *Vacuists* will tell Mr. *Hobbes*, that he is fain in two places of the Explication, we have read, to suppose the Plenitude of the World, that is, to beg the thing in question, which 'tis not to be presum'd they will allow.

A. But may not Mr. *Hobbes* say, that 'tis as lawful for him to suppose a *Plenum*, as for them to suppose a *Vacuum*.

B. I think he may justly say so; but 'tis like they will reply, that, in their way of explicating the Torricellian Experiment, they do not suppose a *Vacuum* as to Air, but prove it. For they shew a great space, that having been just before fill'd with Quicksilver, is now deserted by it, though it appeared not, that any Air succeeded in its room; but rather, that the upper end of the Tube is either totally or near totally so devoid of Air, that the Quicksilver may without resistance, by barely inclining the Tube, be made to fill it to the very top: Whereas Mr. Hobbes is fain to have recourse to that which he knows they deny, the Plenitude of the World, not proving by any sensible Phænomena, that there *did* get in through the Quicksilver Air enough to fill the deserted part of the Tube, but only concluding, that so much Air *must* have got in there, because, the World being full, it could find no room any where else; which the *Vacuists* will take for no

proof at all, and the *Carteſians*, though *Plenifts*, who admit an Etherial matter capable of paſſing through the pores of Glafs, will, I doubt, look upon but as an improper Explication.

**A.** I remember on this occaſion another Experiment of yours, that ſeems unfavourable enough to Mr. *Hobbes's* Explication, and you will perhaps call it to mind when I tell you, that 'twas made in a bended Pipe almoſt fill'd with Quicksilver.

**B.** To ſee whether we understand one another, I will briefly deſcribe the Inſtrument I think you mean. We took a Cylindrical Pipe of Glafs, cloſ'd at the upper end, and of that length, that being dexterouſly bent at ſome Inches from the bottom, the shorter legg was made as parallel as we could to the longer: In this Glafs we found an expedient, (for 'tis not eaſie to do,) to make the Torricellian Experimēnt, the Quicksilver in the shorter legg ſerving instead of the ſtagnant Quicksilver in the uſual Baroſcope, and the Quicksilver in the longer

longer legg reaching above that in the shorter about eight or nine and twenty Inches. Then , by another artifice , the shorter legg , into which the Mercury did not rise within an Inch of the top , was so order'd , that it could in a trice be Hermetically seal'd , without disordering the Quicksilver. And this is the Instrument that I ghesst you mean.

A. It is so , and I remember , that it is the same with that , which in the Paradox about Suction you call , whilst the shorter legg remains unseal'd , a *Travelling Baroscope*. But when I saw you make the Experiment , that legg was Hermetically seal'd , an Inch of Air in its natural or usual consistence being left in the upper part of it , to which Air you outwardly applied a pair of heated Tongs.

B. Yet that , which I chiefly aim'd at in the Trial , was not the Phænomenon I perceive you mean ; for , my design was , by breaking the Ice for them , to encourage some , that may have

have more skill and accommodation than I then had, to make an attempt that I did not find to have been made by any; namely, to reduce the Expansive force of Heat in every way included Air, if not in some other Bodies also, to some kind of measure, and, if 'twere possible, to determin it by weight. And I presumed, that at least the event of my Tryal would much confirm several Explications of mine, by shewing, that Heat is able, as long as it lasts, very considerably to increase the Spring or pressing power of the Air. And in this conjecture I was not mistaken; for, having shut up, after the manner newly recited, a determinate quantity of uncompreſt Air, which, (in the Experiment you ſaw,) was about one Inch; we warily held a pair of heated Tongs near the outside of the Glafs, (without making it touch the Instrument, for fear of breaking it,) whereby the Air being agitated was enabled to expand it ſelf to double its former Dimensions, and conſequently

quently had its Spring so strengthen'd by Heat, that it was able to raise all the Quicksilver in the longer legg, and keep up or sustain a Mercurial Cylinder of about nine and twenty Inches high, when by its expansion it would, if it had not been for the Heat, have lost half the force of its elasticity. But whatever I design in this Experiment, pray tell me, what use you would make of it against Mr. Hobbes.

A. I believe, he will find it very difficult to shew, what keeps the Mercury suspended in the longer legg of the *Travelling Baroscope*, when the shorter legg is unstoppt, at which it may run out; since this Instrument may, as I have try'd, be carried to distant places, where it cannot with probability be pretended, that any Air has been displac'd by the fall of the Quicksilver in the longer legg, which perhaps fell long before above a mile off. And when the shorter legg is seal'd, it will be very hard for Mr. Hobbes to shew there the odd motions

motions of the Air , to which he ascribes the Torricellian Experiment. For , if you warily incline the Instrument , the Quicksilver will rise to the top of the longer legg , and immediately subside , when the Instrument is again erected , and yet no Air appears to pass through the Quicksilver interpos'd between the ends of the longer and the shorter legg. But that which I would chiefly take notice of in the Experiment , is , that upon the external application of a hot Body to the shorter legg of the Baroscope , when 'twas seal'd up , the included Air was expanded from one Inch to two , and so rais'd the whole Cylinder of Mercury in the longer legg , and , whilst the heat continued undiminished , kept it from subsiding again. For , if the Air were able to get unseen through the body of the Quicksilver , why had it not been much more able , when rarified by Heat , to pass through the Quicksilver , than for want of doing so to raise and sustain so weighty a Cylinder

der of Mercury? I shall not stay to inquire on this occasion, how Mr. *Hobbes* will, according to his *Hypothesis*, explicate the rarefaction of the Air to double its former dimensions, and the condensation of it again; especially since, asserting that part of the upper legg, that is unfill'd with the Quicksilver, to be perfectly full of Air, he affirms that, which I doubt he cannot prove, and which may very probably be disproved by the Experiment you mention in the Discourse about *Suction*, where you shew, to another purpose, that in a *Travelling Baroscope*, whose shorter legg is seal'd, if the end of the longer legg be open'd, whereby it comes indeed to be fill'd with Air, the pressure of that Air will enable the subjacent Mercury notably to compress the Air included in the shorter legg.

B. I leave Mr. *Hobbes* to consider what you have objected against his Explication of the Torricellian Experiment; to which I shall add nothing,

thing, though perhaps I could add much, because I think it may be well spared, and our Conference has lasted long already.

A. I will then proceed to the last Experiment recited by Mr. Hobbes in his *Problemata de Vacuo*.

A. *Si Phialam, collum habentem longiusculum, eandemque omni Corpore prater Aerem vacuam ore sugas, continuoque Phiale os aquae immegas, videbis aquam aliquousque ascendere in Phialam. Qui fieri hoc potest nisi factum sit Vacuum ab exsuctione Aeris, in cuius locum possit Aqua illa ascendere?*

B. Concesso Vacuo, oportet quedam loca vacua fuisse in illo Aere, etiam qui erat intra Phialam ante suctionem. Cur ergo non ascendebat Aqua ad ea impleenda absque suctione? Is qui sugit Phialam, neque inventrem quicquam, neque in pulmones, neque in os è Phiala exigit. Quid ergo agit? Aerem commovet, & in partibus ejus conatum sugendo efficit per os excundi, & non admittendo, conatum redeundi. Ab his contrariis componitur circumstatio intra Phialam,

Phialam, & conatus exenndi quaqua-  
versum. Itaque Phiale ore aquae im-  
merso, Aer in subjectam aquam penetrat  
& Phiala egrediens, & tantundem aqua  
in Phialam cogit.

Præterea vis illa magna suctionis  
facit, ut fugentis labra cum collo Phiale  
aliquando arctissime cohereant propter  
contactum exquisitissimum.

B. As to the first Clause of  
Mr. Hobbes's account of our Phæno-  
menon, the *Vacuists* will easily an-  
swer his Question by acknowledging,  
that there were indeed interspers'd  
Vacuities in the Air contain'd in the  
Vial before the suction; but they will  
add, there was no reason, why the  
Water should ascend to fill them, be-  
cause, being a heavy body, it can-  
not rise of it self, but must be raised  
by some prevalent weight or pres-  
sure, which then was wanting. Be-  
sides, that there being interspers'd  
Vacuities as well in the rest of the  
Air that was very near the Water,  
as in that contained in the Vial, there  
was no reason, why the Water  
should

should ascend to fill the Vacuities of one portion of Air rather than those of another. But when once by suction a great many of the Aerial Corpuscles were made to pass out of the Vial ; the Spring of the remaining Air being weaken'd , whilst the pressure of the ambient Air , which depends upon its constant Gravity , is undiminished , the Spring of the internal becomes unable to resist the weight of the external Air , which is therefore able to impel the interpos'd Water with some violence into the Cavity of the Glass , 'till the Air , remaining in that Cavity , being reduced almost to its usual Density , is able by its Spring , and the weight of the Water got up into the Vial , to hinder any more Water from being impell'd up . For , as to what Mr. Hobbes affirms , that , *Is qui sugit Phialam neque in ventrem quicquam , neque in pulmones , neque in os quicquam exigit :* How it will agree with what he elsewhere delivers about Suction , I leave him to consider .

But

But I confess, I cannot but wonder at his confidence, that can positively assert a thing so repugnant to the common sentiments of Men of all opinions, without offering any proof for it. But I suppose, they that are by tryal acquainted with Sucking, and have felt the Air come in at their mouths, will prefer their own experience to his authority. And as to what he adds, that the Person that sucks agitates the Air, and turns it within the Vial into a kind of circulating wind, that endeavours every where to get out; I wish, he had shewn us by what means a Man that sucks makes this odd Commotion of the Air; especially in such Vials as I use to employ about the Experiment, the orifice of whose neck is sometimes less than a Pins head.

A. That there may be really Air extracted by Suction out of a Glass, me thinks you might argue from an Experiment I saw you make with a Receiver which was exhausted by your Pump, and consequently by Suction.

tion. For I remember, when you had counterpois'd it with very good Scales, and afterwards by turning a stop-cock, let in the outward Air, there rush'd in as much Air to fill the space that had been deserted by the Air pump't out, as weighed some scruples (consisting of twenty grains a piece) though the Receiver were not of the largest size.

B. You did well to add that Clause; for, the Magdeburgic Experiment, mentioned by the industrious *Schottus*, having been made with a vast Receiver, the readmitted Air amounted to a whole ounce and some drachms. But to return to Mr. *Hobbes*, I fear not that he will perswade you, that have seen the Experiment he recites, that as soon as the neck of the Vial is unstop'd under water, the Air, that whirl'd about before, makes a sally out, and forces in as much wa-  
ter. For, if the orifice be any thing large, you will, instead of feeling an endeavour to thrust away your finger that stopt it, find the pulp of  
your

your finger so thrust inward, that a *Peripatetick* would affirm that he felt it suckt in. And that *Intrusion* may be the Reason, why the lip of him that sucks is oftentimes strongly fasten'd to the orifice of the Vials neck, which Mr. *Hobbes* ascribes to a most exquisit contact, but without clearly telling us, how that extraordinary contact is effected. And when your finger is removed, instead of perceiving any Air go out of the Vial through the water, (which, if any such thing happen, you will easily discover by the bubbles,) you shall see the water briskly spring up in a slender stream to the top of the Vial, which it could not do, if the Cavity were already full of Air. And to let you see, that, when the Air does really pass in or out of the Vial immers'd under water, 'tis very easie to perceive its motions, if you dip the neck of the Vial in water, and then apply to the globulous part of it either your warm hands or any other competent Heat, the internal

Air being rarified; you shall see a portion of it, answerable to the degree of Heat you applied, manifestly pass through the water in successive bubbles, whilst yet you shall not see any water get into the Vial to supply the place deserted by that Air. And if, when you have (as you may do by the help of sucking) fill'd the neck and part of the belly of the Vial with water, you immerse the orifice into stagnant water, and apply warm hands to the globulous part as before, you will find the water in the Vial to be driven out, before any bubbles pass out of the Vial into the surrounding water; which shews, that the Air is not so forward to dive under the water, (and much less under so ponderous a liquor as Quick-silver,) as Mr. Hobbes has supposed.

A. That 'tis the Pressure of the external Air, that (surmounting the Spring of the internal) drives up the water into the Vial we have been speaking of, does, I confess, follow upon your Hypothesis: But an Experimen-

rientalian Philosopher, as Mr. Hobbes calls you among others, may possibly be furnished with an Experiment to confirm this to the Eye.

B. You bring into my mind what I once devised to confirm my Hypothesis about Suction, but found a while since that I had omitted it in my Discourse about that Subject. And therefore I shall now repeat to you the substance at least of the Memorial that was written of that Experiment, by which the great interest of the weight of the Atmospheric Air in Suction will appear, and in which also some things will occur, that will not well agree with Mr. Hobbes's Explication, and prevent some of his Allegations against mine.

A. Having not yet met with an Experiment of this nature, such an one as you speak of will be welcome to me.

B. We took a Glass Bubble, whose long stem was both very slender and very Cylindrical; then by applying

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to the outside of the Ball or globulous part a convenient heat, we expell'd so much of the Air, as that, when the end of the pipe was dipt in water, and the inward Air had time to recover its former coolness, the water ascended either to the top of the pipe or very near it. This done, we gently and warily rarified the Air in the Cavity of the Bubble, 'till by its expansion it had driven out almost all the water that had got up into the stem, that so it might attain as near as could be to that degree of heat and measure of expansion, that it had when the water began to rise in it. And we were careful to leave two or three drops of water unexpell'd at the bottom of the pipe, that we might be sure, that none of the included Air was by this second rarefaction driven out at the orifice of it; as the depression of the water so low assured us, on the other side, that the included Air wanted nothing considerable of the expansion it had when the water began to ascend into the

the pipe. Whilst the Air was in this rarified state , we presently removed the little Instrument out of the stagnat water into stagnat Quicksilver , which in a short time began to rise in the pipe. Now , if the ascension of the liquor were the effect of Natures Abhorrence of a *Vacuum* ; or of some internal principle of Motion ; or of the Compression and propagated Pulsion of the outward Air by that which had been expell'd ; why should not the Mercury have ascended to the top of the pipe , as the water did before ? But *de facto* it did not ascend half , or perhaps a quarter so far ; and if the pipe had been long enoughe , as well as 'twas slender enoughe , I question , whether the Mercury would have ascended (in proportion to the length of the stem) half so high as it did .

Now of this Experiment , which we tryed more than once , I see not ; for the reason lately expres'd , how any good account will be given without our *Hypothesis* , but according to That 'tis clear .

A. I think I perceive why you say so; for the Ascension of Liquors being an effect of the prevalency of the external Airs pressure against the resistance it meets with in the Cavity of the Instrument, and the Quicksilver being bulk for bulk many times heavier than water, the same surplusage of pressure that was able to impel up water to the top of the pipe, ought not to be able to impel up the Quicksilver to any thing near that height. And if it be here objected, as it very plausibly may be, that the raised Cylinder of Mercury was much longer than it ought to have been in reference to a Cylinder of Water, the proportion in gravity between those two Liquors (which is almost that of fourteen to one) being considered; I answer, that when the Cylinder of Water reach'd to the pipe, the Air possess'd no more than the Cavity of the globulous part of the Instrument, being very little assisted to dilate it self by so light a Cylinder as that of Water: But when

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the Quicksilver came to be impell'd into the Instrument by the weight of the external Air, that ponderous Body did not stop its ascent as soon as it came to be equiponderant to the formerly expell'd Cylinder of Water; because, to attain that height, it reached but a little way into the pipe, and left all the rest of the Cavity of the pipe to be fill'd with part of that Air, which formerly was all shut up in the Cavity of the Bubble; by which means the Air, included in the whole Instrument, must needs be in a state of expansion, and thereby have its Spring weakened, and consequently disabled to resist the pressure of the external Air, as much as the same included Air did before, when it was less rarified; on which account, the undiminished weight or pressure of the external Air was able to raise the Quicksilver higher and higher, 'till it had obtained that height, at which the pressure, compounded of the weight of the Mercurial Cylinder and the Spring of the internal

internal Air (now less rarified than before,) was equivalent to the pressure of the Atmosphere or external Air.

B. You have given the very Explication I was about to propose; wherefore I shall only add, that, to confirm this Experiment by a kind of Inversion of it, we drove by heat a little Air out of the Bubble, and dipt the open end of the pipe into Quicksilver, which by this means we made to ascend 'till it had fill'd about a fourth part or less of the pipe, when that was held erected. Then carefully removing it without letting fall any Quicksilver, or letting in any Air, we held the orifice of the pipe a little under the surface of a Glaſs full of Water, and applying a moderate heat to the outside of the Ball, we warily expell'd the Quicksilver, yet leaving a little of it to make it sure that no Air was driven out with it; then suffering the included Air to cool, the external Air was found able to make the

Water

Water not only ascend to the very top of the pipe, and thence spread it self a little into the Cavity of the Ball, but to carry up before it the Quicksilver that had remained unexpell'd at the bottom of the stem. And if in making the Experiment we had first raised, as we sometimes did, a greater quantity of Quicksilver, and afterwards drove it out, the quantity of Water, that would be impell'd into the Cavity of the pipe and ball, would be accordingly increased.

A. In this Experiment 'tis manifest, that something is driven out of the Cavity of the Glass before the Water or Quicksilver begins to ascend in it: And here also we see not, that the Air can pass through the pores of Quicksilver or Water, but that it drives them on before it, without sensibly mixing with them. In this Experiment there appears not at all any Circular Wind, as Mr. Hobbes fancies in the sukt Vial we are disputing of, nor any tendency outwards  
of

of the included Air upon the account of such a Wind; but, instead of these things, that the ascension of the Liquors into the Cavity of the pipe depends upon the external Air, pressing up the Liquors into that Cavity, may be argu'd by this, that the same weight of the Atmosphere impell'd up into the pipe so much more of the lighter Liquor, *water*, than of the heavier Liquor, *Mercury*.

B. You have said enough on this Experiment; but 'tis not the only I have to oppose to Mr. Hobbes his Explication: For, that there is no need of the sallying of Air out of a Vial, to make the Atmospheric Air press against a Body that closes the orifice of it, when the pressure of the internal Air is much weakened; I have had occasion to shew some *Virtuosi*, by sucking out, with the help of an Instrument, a considerable portion of the Air contained in a Glaſs; for having then, instead of unstopping the orifice under water, nimbly applied a flat Body to it, the external Air press'd that

that Body so forcibly against it, as to keep it fastened and suspended, though 'twere clogg'd with a weight of many ounces.

A. Another Experiment of yours Mr. *Hobbes*'s Explication brings into my mind, by which it appears, that, if there be such a Circular Wind, as he pretends, produced by Suction in the Cavity of the Vial, it must needs be strangely lasting. For I have seen more than once, that, when you have by an Instrument suckt much of the Air out of a Vial, and afterwards carefully closed it, though you kept the slender neck of it stopt a long time, perhaps for some weeks or months, yet when 'twas open'd under water, a considerable quantity of the Liquor would be briskly impell'd up into the neck and belly of the Vial. So that, though I will not be so pleasant with Mr. *Hobbes*, as to mind you on this occasion of those Writers of Natural Magick, that teach us to shut up Articulate Sounds in a Vessel, which being transported to a distant place

place and open'd there , will render the Words that are committed to it ; yet I must needs say , that so lasting a Circular Wind , as , according to Mr. Hobbes , your Experiments exhibited , may well deserve our wonder .

E. Your admiration would perchance increase , if I should assure you , that having with the Sun-beams produced smoak in one of those well-stopt Vials , this Circular Wind did not at all appear to blow it about , but suffered it to rise , as it would have done if the included Air had been very calm . And now I shall add but one Experiment more , which will not be liable to some of the things as invalid as they are , which Mr. Hobbes has alledged in his account of the Vial , and which will let you see , that the weight of the Atmospherical Air is a very considerable thing ; and which may also incline you to think , that , whilst Mr. Hobbes does not admit a subtler Matter than common Air to pass through the Pores of close and solid Bodies , the Air he has recourse to will some-

sometimes come too late to prevent a *Vacuum*. The Experiment, which was partly accidental, I lately found registred to this sense, if not in these words: [Having, to make some Discovery of the weight of the Air, and for other purposes, caus'd an *Æolipile*, very light considering its bulk, to be made by a famous Artist, I had occasion to put it so often into the fire for several Tryals, that at length the Copper scal'd off by degrees, and left the Vessel much thinner than when it first came out of the Artificers hands; and a good while after, this change in the Instrument being not in my thoughts, I had occasion to employ it, as formerly, to weigh how many grains it would contain of the Air at such a determinate constitution of the Atmosphere, as was to be met with, where I then chanced to be. For the making this Experiment the more exactly, the Air was by a strong, but warily applied, fire so carefully driven away, that, when clapping a piece of Sealing-wax to the Pin-hole, at which it had been forced out, we hindred any

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communication betwixt the Cavity of the Instrument and the external Air, we suppos'd the Æolipile to be very well exhausted, and therefore laid it by, that, when it should be grown cold, we might, by opening the orifice with a Pin, again let in the outward Air, and observe the encrease of weight that would thereupon ensue: But the Instrument, that, as I was saying, was grown thin, had been so diligently freed from Air, that the very little that remain'd, and was kept by the Wax from receiving any assistance from without, being unable by its Spring to assist the Æolipile to support the weight of the ambient Air; this external fluid did by its weight press against it so strongly, that it compress'd it, and thrust it so considerably inwards, and in more than one place so chang'd its figure, that, when I shew'd it to the *Virtuosi* that were assembled at *Gresham-Colledge*, they were pleased to command it of me to be kept in their Repository, where I presume it is still to be seen.

*F I N I S.*



OF THE  
CAUSE  
OF  
ATTRACTION  
BY  
Suction,  
A  
PARADOX.

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By the Honourable  
ROBERT BOYLE,  
Fellow of the Royal Society.

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LONDON,  
Printed by William Godbid, and are to  
be Sold by Moses Pitt, at the Angel over  
against the little North Door of  
St. Paul's Church. 1674,



## PREFACE.

Having about twelve years ago summarily express'd and publish'd my Opinion of the Cause of Suction, and a while before or after brought to the Royal Society the Glass Instrument I employ'd to make it out; I desisted for some time to add any thing about a Problem, that I had but occasionally handled: Only, because the Instrument I mention'd in my Examen of Mr. Hobbes's Opinion, and afterwards us'd at Gresham-College, was difficult enough to be well made, and not to be procur'd ready made, I did for the sake of some Virtuosi, that were curious of such things, devise a slight and easily made Instrument, describ'd in the following Tract, Chap. 4<sup>th</sup>, in which the chief Phænomena, I shew'd before the Society, were easily producible. But afterwards the mistakes and erroneous Opinions,

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## PREFACE.

that, in Print as well as in Discourse, I met with, even among Learned Men, about Suction, and the Curiosity of an Ingenious Person, engaged me to resume that Subject and treat of it, as if I had never before meddled with it, for the reason intimated in the beginning of the insuing Paper. And finding upon the review of my later Animadversions on Mr. Hobbes's Problemata de Vacuo, that some passages of this Tract are referr'd to there; I saw my self thereby little less than engaged to annex that Discourse to those Animadversions. And this I the rather consented to, because it contains some Experiments, that I have not elsewhere met with, which, together with some other parts of that Essay, may, I hope, prove of some use to illustrate and confirm our Doctrine about the Weight and Spring of the Air, and supply the less experienced than ingenious Friends to our Hypothesis with more grounds of answering the later Objections of some Learned Men, against whose endeavours I perceive it will be useful to employ variety of Experiments and other Proofs to evince

## PREFACE.

winne the same Truth ; that some or other of these may meet with those Arguments or evasions with which they strive to elude the force of the rest.

The Title of the following Essay may sufficiently keep the Reader from expecting to find any other kind of Attraction discours'd of, than that which is made by Suction. But yet thus much I shall here intimate in general, that I have found by Trials purposely made, that the Examples of Suction are not the only noted ones of Attraction , that may be reduced to Pulsion.

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OF THE  
CAUSE  
OF  
Attraction by Suction,  
A PARADOX.

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CHAP. I.

I Might, Sir, save my self some trouble in giving you that account you desire of me about *Suction*, by referring you to a passage in the *Examen*, I long since writ, of Mr. Hobbes's *Dialogus Physicus de Natura Aeris*, if I knew, you had those two Books lying by you. But because I suspect, that my *Examen*

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may not be in your hands, since 'tis almost out of Print, and has not for some years been in my own; and because I do not so well remember, after so long a time, the particulars that I writ there, about Suction, as I do in general, that the *Hypothesis* I proposed, was very incidentally and briefly discours'd of, upon an occasion ministred by a wrong Explication given of Suction by Mr. Hobbes, I shall here decline referring you to what I there writ; and proposing to you those thoughts about Suction, that I remember I there pointed at, I shall annex some things to illustrate and confirm them, that would not have been so proper for me to have insisted on in a short and but occasional Excursion.

And I should immediately proceed to what you expect from me, but that *Suction* being generally look'd upon as a kind of *Attraction*, it will be requisite for me to premise something about *Attraction* it self. For, besides that the Cause of it, which

I here dispute not of, is obscure, the very Nature and Notion of it is wont by Naturalists to be either left untouched, or but very darkly deliver'd, and therefore will not be unfit to be here somewhat explain'd.

How general and ancient soever the common Opinion may be, that *Attraction* is a kind of Motion quite differing from *Pulsion*, if not also opposite to it; yet I confess, I concur in opinion, though not altogether upon the same grounds, with some modern Naturalists, that think Attraction a *Species* of Pulsion. And at least among inanimate Bodies I have not yet observed any thing, that convinces me, that Attraction cannot be reduced to Pulsion; for, these two seem to me to be but extrinsical denominations of the same Local Motion, in which, if a moved Body *precede* the Movent, or tend to acquire a greater distance from it, we call it *Pulsion*; and if, upon the score of the Motion, the same Body *follow* the Movent or approach to it, we call it

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*Attraction.* But this difference may consist but in an *accidental* respect, which does not *Physically* alter the nature of the Motion, but is founded upon the respect, which the Line, wherein the Motion is made, happens to have to the situation of the *Movent*. And that which seems to me to have been the chief cause of mens mistaking Attraction for a motion opposite to Pulsion, is, that they have look'd upon both the moving and moved Bodies, in too popular and superficial a manner; and consider'd in the *Movent* rather the situation of the conspicuous and more bulky part of the Animal or other Agent, than the situation of that part of the Animal, or Instrument, that does immediately impress that motion upon the *Mobile*.

For those that attentively heed this, may easily take notice, that some part of that Body, or of the Instrument, which by reason of their conjunction in this operation is to be look'd on but as making one with it, is really placed

placed behind some part of the Body to be drawn, and therefore cannot move outwards it self without thrusting that Body forward. This will be easily understood, if we consider, what happens when a Man draws a Chain after him; for, though his Body do *precede* the Chain, yet his finger or some other part of the hand, wherewith he draws it, has some part or other that reaches *behind* the fore part of the first Link, and the hinder part of this Link comes behind the anterious part of the second Link; and so each Link has one of its parts placed behind some part of the Link next after it, 'till you come to the last Link of all. And so, as the finger, that is in the first Link, cannot move forwards but it must thrust on that Link, by this *series* of Trusions the whole Chain is moved forwards; and if any other Body be drawn by that Chain, you may perceive, that some part of the last Link comes behind some part of that Body, or of some intervening Body,

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which, by its cohesion with it, ought in our present case to be consider'd as part of it. And thus *Attraction* seems to be but a *Species* of *Pulsion*, and usually belongs to that kind of it, which, for distinctions sake, is called *Trusion*, by which we understand that kind of Pulsion, wherein the Movent goes along with the moved Body without quitting it whilst the progresſ lasts; as it happens, when a Gardiner drives his Wheel-barrow before him without letting go his hold of it.

But I must not here dissemble a difficulty, that I foresee may be speciously urged against this account of Attraction. For it may be said, that there are Attractions, where it cannot be pretended, that any part of the *Attrahent* comes behind the *Attracted* Body; as in *Magnetical* and *Electrical* Attractions, and in that which is made of Water, when 'tis drawn up into Springs and Pumps.

I need not tell you, that you know so well, as that partly the *Cartesians*, and

and partly other Modern Philosophers, have recourse on this occasion either to *screwed Particles* and other Magnetical Emissions, to explicate *Phænomena* of this kind. And, according to such *Hypotheses*, one may say, that many of these Magnetical and Electrical *Effluvia* come behind some parts of the attracted Bodies, or at least of the little solid Particles, that are as it were the Walls of their Pores, or procure some discussion of the Air, that may make it thrust the Moveable towards the Loadstone or Amber, &c. But if there were none of these, nor any other subtil Agents that cause this Motion by a real, though unperceived, Pulsion; I should make a distinction betwixt other Attractions and these, which I should then stile *Attraction by Invisibles*. But, whether there be really any such in Nature, and why I scruple to admit things so hard to be conceived, may be elsewhere consider'd. And you will, I presume, the freelier allow me this liberty, if, (since

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in this place 'tis proper to do it,) I  
shew you, that in the last of the in-  
stances I formerly objected (that of  
the drawing up of Water into the  
Barrel of a Syringe,) there is no true  
Attraction of the Liquor made by the  
external Air. I say then, that by  
the ascending Rammer, as a part of  
which I here consider the obtuse end,  
Plug, or Sucker, there is no Attra-  
ction made of the contiguous and sub-  
jacent Water, but only there is room  
made for it, to rise into, without  
being expos'd to the pressure of the  
superior Air. For, if we suppose  
the whole Rammer to be by Divine  
Omnipotence annihilated, and con-  
sequently uncapable of exercising any  
Attraction; yet, provided the su-  
perior Air were kept off from the Wa-  
ter by any other way as well as 'twas  
by the Rammer, the Liquor would  
as well ascend into the Cavity of  
the Barrel; since, (as I have else-  
where abundantly proved,) the sur-  
face of the Terraqueous Globe being  
continually press'd on by the incum-  
bent

bent Air or Atmosphere, the Water must be by that pressure impell'd into any cavity here below, where there is no Air to resist it; as by our Supposition there is not in the Barrel of our Syringe, when the Rammer, or whatever else was in it, had been annihilated. Which Reasoning may be sufficiently confirm'd by an Experiment, whereby I have more than once shewn some curious persons, that, if the external Air, and consequently its pressure, be withdrawn from about the Syringe, one may pull up the Sucker as much as he pleases, without drawing up after it the subjacent Water. In short, let us suppose, that a Man standing in an inner room does by his utmost resistance keep shut a Door, that is neither lock'd nor latch'd, against another, who with equal force endeavours to thrust it open: In this case, as if one should forcibly pull away the first Man, it could not be said, that this Man, by his recess from the Door he endeavoured to pres

press outwards ; did truely and properly draw in his Antagonist, though upon that receſt the coming in of his Antagonist would presently ensue ; ſo it cannot properly be ſaid, that by the ascent of the Rammer, which displaces the ſuperior Air, either the Rammer it ſelf, or the expelled Air, does properly *attract* the ſubjacent Water, though the ingress of that Liquor into the Barrel does thereupon neceſſarily ensue. And that, as the Comparison ſuppoſes, there is a pressure of the ſuperior Air againſt the upper part of the Sucker, you may eaſily perceive, if having well ſtopt the lower orifice of the Syringe with your finger, you forcibly draw up the Sucker to the top of the Barrel. For if then you let go the Rammer, you will find it impell'd downwards by the incumbent Air with a notable force.

## C H A P. II.

Having thus premis'd something in general about the Nature of *Attraction*, as far as 'tis necessary for my present design; it will be now seasonable to proceed to the consideration of that kind of Attraction, that is employed to raise Liquors, and is by a distinct Name called *Suction*.

About the Cause of this there is great contention between the New Philosophers, as they are stiled, and the Peripateticks. For the Followers of *Aristotle*, and many Learned Men that in other things dissent from him, ascribe the ascension of Liquors upon *Suction* to Natures abhorrence of a *Vacuum*. For, say they, when a Man dips one end of a Straw or Reed into stagnant Water, and sucks at the other end, the Air contain'd in the cavity of the Reed passes into that

of

of his Lungs , and consequently the Reed would be left empty , if no other Body succeeded in the place it deserts ; but there are only (that they take notice of,) two Bodies that can succeed , the Air and the (grosser Liquor) the Water ; and the Air cannot do it , because of the interposition of the Water , that denies it access to the immers'd orifice of the Reed , and therefore it must be the Water it self , which accordingly does ascend to prevent a *Vacuum* detested by Nature.

But many of the Modern Philosophers , and generally all the *Corpuscularians* , look upon this *Fuga Vacui* as but an imaginary Cause of *Suction* , though they do it upon very differing grounds. For , the *Atomists* , that willingly admit of Vacuities , properly so called , both within and without our World , cannot think that Nature hates or fears a *Vacuum* , and declines her usual course to prevent it : And the *Cartesians* , though they do , as well as the *Peripateticks* , deny

deny that that there is a *Vacuum*, yet since they affirm not only, that there is none *in rerum Natura*, but that there *can be* none, because what others call an empty Space having three Dimensions, hath all that they think belonging to the Essence of a Body, they will not grant Nature to be so indiscreet, as to strain her self to prevent the making of a thing that is impossible to be made.

The *Peripatetic* Opinion about the Cause of Suction, though commonly defended by the Schools, as well Modern as Ancient, supposes in Nature such an abhorrence of a *Vacuum*, as neither has been well proved, nor does well agree with the lately discover'd Phænomenon of *Suction*. For, according to their *Hypothesis*, Water and other Liquors should ascend upon Suction to any hight to prevent a *Vacuum*, which yet is not agreeable to experience. For I have carefully tryed, that by pumping with a Pump far more stanch than those that are usually made, and indeed as well clos'd

clos'd as we could possibly bring it to be, we could not by all our endeavours \*See Cont. of raise Water by Suction to *Phys. Mech. Exp.* above \* $36\frac{1}{2}$  foot. The *Torricellian Exp<sup>t</sup>* shews, that the weight of the Air is able to sustain, and some of our Experim<sup>ts</sup> shew, 'tis able to raise a Mercurial Cylinder equal in weight to as high a Cylinder of Water as we were able to raise by pumping. For Mercury being near 14 times as heavy as Water of the same bulk, if the weight of the Air be equivalent to that of a Mercurial Cylinder of 29 or 30 Inches, it must be able to counterpoise a Cylinder of Water near fourteen times as long, that is, from thirty four to near thirty six foot. And very disagreeable to the common *Hypothesis*, but consonant to ours, is the Experiment that I have more than once tryed, and I think elsewhere deliver'd, namely, That; if you take a Glas Pipe of about three foot long, and, dipping one end of it in Water, suck at the other, the Water will be suddenly made

made to flow briskly into your mouth: But, if instead of Water you dip the lower end into Quicksilver, though you suck as strongly as ever you can, provided that in this case, as in the former, you hold the Pipe upright, you will never be able to suck up the Quicksilver near so high as your mouth; so that if the Water ascended upon Suction to the top of the same Pipe, because else there would have been a *Vacuum* left in the cavity of it, why should not we conclude, that, when we have suckt up the Quicksilver as strongly as we can, as much of the upper part of the Tube as is deserted by the Air, and yet not fill'd by the Mercury, admits, in part at least, a *Vacuum*, (as to Air) of which consequently Nature cannot reasonably be suppos'd to have so great and unlimited an abhorrency, as the *Peripateticks* and their Adherents presume. Yet I will not determine, whether there be any more than many little Vacuities, or Spaces devoid of Air, in the Cavity,

so called, of the Pipe unfill'd by the Mercury ; (so that the whole Cavity is not one entire empty Space ; ) it being sufficient for my purpose, that my Experiment affords a good Argument *ad hominem* against the Peripateticks, and warrants us to seek for some other Cause than the *fuga Vacui*, why a much stronger Suction than that, which made Water ascend with ease into the Suckers mouth , will not also raise Quicksilver to the same height or near it.

Those Modern Philosophers that admit not the *fuga Vacui* to be the Cause of the raising of Liquors in Suction , do generally enough agree in referring it to the action of the Suckers *thorax*. For, when a Man endeavours to suck up a Liquor, he does by means of the Muscles enlarge the cavity of his Chest , which he cannot do but at the same time he must thrust away those parts of the ambient Air that were contiguous to his Chest , and the displac'd Air does, according to some Learned Men ,  
(therein,

(therein, if I mistake not, Followers of *Gassendus*,) compress the contiguous Air, and that the next to it, and so outwards, 'till the pressure, successively passing from one part of the Air to the other, arrive at the surface of the Liquor; and all other places being as to sense full, the impell'd Air cannot find place but by thrusting the Water into the room made for it in the Pipe by the receſs of the Air that pass'd into the Suckers lungs. And they differ'd not much from this Explication, that, without taking in the compression of the ambient Air made by the *thorax*, refer the Phænomenon to the propagated motion or impulse, that is impreſt on the Air displac'd by the *thorax* in its dilatation, and yet unable to move in a World perfectly fill'd, as they suppose ours to be, unless the Liquor be impell'd into as much of the cavity of the Pipe, as fast as 'tis deserted by the Air that is said to be suck'd up. But though I readily confess this Explication to be ingenious, and such as I

wonder not they should acquiesce in, who are acquainted but with the long known and obvious *Phænomena* of Suction; and though I am not sure, but that in the most familiar cases the Causes assign'd by them may contribute to the Effect; yet, preserving for *Cartesius* and *Gassendus* the respect I willingly pay such great Philosophers, I must take the liberty to tell you, that I cannot acquiesce in their Theory. For I think, that the Cause of *Suction*, they assign, is in many cases not necessary, in others, not sufficient. And first, as to the Condensation of the Air by the dilatation of the Suckers Chest; when I consider the extent of the ambient Air, and how small a compression no greater an expansion than that of the *Thorax* is like to make, I can scarce think, so slight a condensation of the free Air can have so considerable an operation on the surface of the Liquor to be rais'd, as the *Hypothesis* I examin requires: And that this impulse of the Air by a Suckers dilated

dilated *Thorax*, though it be wont to accompany the ascension of the water procured by Suction, yet is not of absolute necessity to it, will, I presume, be easily granted, if it can be made out, that even a propagated *Pulsion*, abstracted from any Condensation of Air, is not so necessarily the Cause of it, but that the Effect may be produc'd without it. For suppose, that by Divine Omnipotence so much Air as is displac'd by the *Thorax* were annihilated; yet I see not, why the Ascension of the Liquor should not ensue. For, when a Man begins to suck, there is an *Æquilibrium*, or rather *Æquipollency* between the pressure, which the Air, contained in the Pipe, (which is shut up with the pressure of the Atmosphere upon it,) has, by virtue of its *Spring*, upon that part of the surface of the water that is environ'd by the sides of the Pipe, and the pressure which the Atmospheric Air has, by virtue of its *weight*, upon all the rest of the surface of the stagnant water; so that,

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when by the dilatation of the Suc-  
kers *Thorax*, the Air within the ca-  
vity of the Pipe comes to be rarified,  
and consequently loose of its *Spring*,  
the weight of the external Air conti-  
nuing in the mean time the same, it  
must necessarily happen, that the  
Spring of the internal Air will be  
too weak to compress any longer the  
gravitation of the external, and con-  
sequently, that part of the surface of  
the stagnant water, that is included  
in the Pipe, being less press'd upon,  
than all the other parts of the same  
surfaces must necessarily give way,  
where it can least resist, and con-  
sequently be impell'd up into the Pipe,  
where the Air, having had its Spring  
weakened by expansion, is no lon-  
ger able to resist, as it did before.  
This may be illustrated by somewhat  
varying an Instance already given,  
and conceiving, that within a Cham-  
ber three Men thrust all together with  
their utmost force against a Door,  
(which we suppose to have neither  
Dolt nor Latch) to keep it shut, at  
the

the same time the three other Men have just equal strength, and employ their force to thrust it open. For though , whilst their opposite endeavours are equal , the Door will continue to be kept shut, yet if one of the three Men within the Room should go away, there will need no new force, nor other accession of strength to the three Men, to make them prevail and thrust open the Door against the resistance of those that endeavour'd to keep it shut, who are now but two,

And here (upon the by) you may take notice , that , to raise water in Suction , there is no necessity of any rarified and forcibly stretch'd Rope, as 'twere , of the Air, to draw up the subjacent water into the Pipe , since the bare debilitation of the Spring of the included Air may very well serve the turn. And though, if we should suppose the Air within the Pipe to be quite annihilated , it could not be pretended (since it would not have so much as Existence) that it exer-

cises an attractive Power; yet in this case the water would ascend into the Pipe, without the assistance of Natures imaginary Abhorrence of a *Vacuum*, but by a Mechanical Necessity, plainly arising from this, that there would be a pressure of the incumbent Atmosphere upon the rest of the surface of the stagnant water, and no pressure at all upon that part of the surface that is within the Pipe, where consequently there could be no resistance made to the ascension of the water, every where else strongly urg'd by the weight of the incumbent Air.

I shall add on this occasion, that, to shew some inquisitive Men, that the weak resistance within a Vessel, that had but one orifice expos'd to the water, may much more contribute to the ascension of that Liquor into the Vessel, than either the compression or the continued or reflected impulse of the external Air; I thought fit to produce a Phænomenon, which by the Beholders was without scruple judg'd

judg'd an Effect of Suction, and yet could not be ascrib'd to the Cause of Suction, assign'd by either of the Sects of Philosophers I dissent from. The Experiment was this: By a way, elsewhere deliver'd, the long neck of a Glass-bubble was seal'd up, and almost all the Air had been by Heat driven out of the whole cavity of the Bubble or Vial, and then the Glass was laid aside for some hours, or as long as we pleas'd; afterwards the seal'd *apex* of the neck was broken off under water: I demand now of a *Peripatetic*, whether the Liquor ought to be suck'd or drawn into the cavity of the Glass, and why? if he says, as questionless he will, that the water would be attracted to hinder a *Vacuum*, he would thereby acknowledge, that, 'till the Glass was unstopt under water, there was some empty space in it; for, 'till the sealed end was broken off, the water could not get in, and therefore, if the *fuga vacui* had any thing to do in the ascension, the Liquor must rise, not to

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prevent an empty space, but to fill one that was made before. Nor does our Experiment much more favour the other Philosophers, I dissent from: For in it there is no dilatation made of the sides of the Glass, as in ordinary Suction there is made of the *Thorax*, but only there is so much Air driven out of the cavity of the Bubble, into whose room since neither common Air nor Water is permitted to succeed, it appears not, how the propagated and returning impulse, or the Circle of Motion, as to common Air and Water, does here take place. And then I demand, what becomes of the Air, that has been by heat driven out, and is by the Hermetical Seal kept out of the cavity of the Bubble? If it be said, that it diffuses it self into the ambient Air, and mingles with it, that will be granted which I contended for, that so little Air as is usually displac'd in Suction cannot make any considerable compression of the free ambient Air; for, what can one Cubic Inch

of

of Air, which is sometimes more than one of our Glasses contains, do, to the condensation so much as of all the Air in the Chamber, when the expell'd Corpuscles are evenly distributed among those of the ambient. And how comes this inconsiderable condensation to have so great an effect in every part of the room, as to be able there to impel into the Glass as much water in extent as the whole Air that was driven out of the cavity of it? But if it be said, that the expell'd Air condens'd only the contiguous or very neighbouring Air, 'tis easie to answer, that 'tis no way probable, that the expell'd Particles of the Air should not by the differing motions of the ambient Air be quickly made to mingle with it, but should rather wait (which if it did we sometimes made it do for many hours) 'till the Vessels whence 'twas driven out were unstopp'd again. But, though this could probably be pretended, it cannot truly be afferted. For if you carry the seal'd Glass quite out of the room

room or house, and unstop it at some other place, though two or three miles distant; the ascension of the water will, (as I found by tryal) nevertheless insue; in which case I presume, it will not be said, that the Air, that was expell'd out of the Glas, and condens'd the contiguous or near contiguous Air, attended the Bubble in all its motions, and was ready at hand to impel in the water, as soon as the seal'd *apex* of the Vial was broken off. But I doubt not, but most of the Embracers of the Opinion I oppose, being Learned and Ingenuous Persons, if they had been acquainted with these and the like *Phenomena*, would rather have changed their Opinion about *Suction*, than have gone about to defend it by such Evasions, which I should not have thought worth proposing, if I had not met with Objections of this nature publickly maintain'd by a Learned Writer, on occasion of the Air's rushing into the exhausted *Magdeburgic Engine*. But as in our Experiment

riment these Objections have no place, so in our *Hypothesis* the Explication is very easie, as will anon be intimated.

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### C H A P. III.

HAVING thus shewn, that the Ascension of Water upon Suction may be caus'd otherwise than by the Condensation or the propagated Pulsion of Air contiguous to the Suckers *Thorax*, and thrust out of place by it; it remains that I shew, (which was one of the two things I chiefly intended,) that there may be Cases wherein the Cause, assign'd in the *Hypothesis* I am examining, will not have place. But this will be better understood, if, before I proceed to the proof of it, I propose to you the thoughts, I had many years since, and do still retain, about the Cause of the Ascension of Liquors in Suction.

To clear the way to the right understanding

derstanding of the ensuing Discourse, it will not be amiss here to premise a summary intimation of some things that are suppos'd in our *Hypothesis*.

We suppose then first, without disputing either the Existence or the nature of Elementary Air, that the Common Air we breath in, and which I often call Atmospherical Air, abounds with Corpuscles not devoid of Weight, and endow'd with Elasticity or Springiness, whereby the lower parts, comprest by the weight of the upper, incessantly endeavour to expand themselves, by which expansion, and in proportion to it, the Spring of the Air is weaken'd, (as other Springs are wont to be) the more they are permitted to stretch themselves.

Next, we suppose, that the Terraqueous Globe, being environ'd with this gravitating and springy Air, has its surface and the Bodies plac'd on it prest by as much of the Atmosphere as either perpendicularly leans on them, or can otherwise come to bear upon

upon them. And this pressure is by the *Torriceilian* and other Experiments found to be equivalent to a perpendicularly erected Cylinder of about twenty nine or thirty Inches of Quicksilver, (for the height is differing, as the gravity of the Atmosphere happens to be various.)

*Lastly*, we suppose, that, Air being contain'd in a Pipe or other hollow Body that has but one orifice open to the free Air, if this orifice be Hermetically seal'd, or otherwise (as with the mouth of one that sucks) clos'd, the now included Air, whilst it continues without any farther expansion, will have an elasticity equivalent to the weight of as much of the outward Air as did before press against it. For, if the weight of the Atmosphere, to which it was then expos'd, had been able to compress it further, it would have done so, and then the closing of the orifice, at which the internal and external Air communicated, as it fenc'd the included Air from the pressure of

of the incumbent, so it hindred the same included Air from expanding it self; so that, as it was shut up with the pressure of the Atmosphere upon it, that is in a state of as great compression as the weight of the Atmosphere could bring it to, so, being shut up and thereby kept from weakening that pressure by expansion, it must retain a Springiness equipollent to the pressure 'twas expos'd to before, which (as I just now noted) was as great as the weight of the incumbent Pillar of the Atmosphere could make it. But if, as was said in the first Supposition, the included Air should come to be dilated or expanded, the Spring being then unbent, its Spring, like that of other elastical Bodies, would be debilitated answerably to that expansion.

To me then it seems, that, speaking in general, Liquors are upon Suction raised into the cavities of Pipes and other hollow Bodies, when, and so far as, there is a less pressure  
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on the surface of the Liquor in the cavity, than on the surface of the external Liquor that surrounds the Pipe, whether that pressure on those parts of the external Liquor, that are from time to time impell'd up into the orifice of the Pipe, proceed from the weight of the Atmosphere, or the propagated compression or impulse of some parts of the Air, or the Spring of the Air, or some other Cause, as the pressure of some other Body quite distinct from Air.

Upon the general view of this *Hypothesis*, it seems very consonant to the Mechanical Principles. For, if there be on the differing parts of the surface of a fluid Body unequal pressures, 'tis plain, as well by the nature of the thing, as by what has been demonstrated by *Archimedes*, and his Commentators, that the greater force will prevail against the lesser, and that that part of the waters surface must give way, where it is least prest. So that that, wherein the *Hypothesis* I venture to propose to you,

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differs from that which I dissent from, is not, that mine is less Mechanical; but *partly* in this, that, whereas the *Hypothesis*, I question, supposes a necessity of the protrusion or impulse of the Air, mine does not require that supposition, but, being more general, reaches to other ways of procuring the Ascension of Liquors, without raising them by the impulse of the Air; and *partly*, and indeed chiefly, in that the *Hypothesis*, I decline, makes the Cause of the Ascension of Liquors to be only the increased pressure of the Air external to the pipe; and I chiefly make it to depend upon the diminished pressure of the Air within the pipe, on the score of the expansion 'tis brought to by Suction.

To proceed now to some Experiments that I made in favour of this *Hypothesis*, I shall begin with that which follows:

We took a Glass-pipe bended like a Syphon, but so that the shorter legg was as parallel to the longer as we could

could get it made, and was Hermetically seal'd at the end: Into this Syphon we made a shift (for 'tis not very easie) to convey water, so that the crooked part being held downwards, the liquor reach'd to the same height in both the leggs, and yet there was about an Inch and half of uncomprest Air shut up in the shorter legg. This little Instrument (for 'twas but about fifteen Inches long) being thus prepar'd, 'tis plain, that according to the *Hypothesis* I dissent from; there is no reason, why the water should ascend upon Suction. For, though we should admit, that the external Air were considerably comprest, or received a notable impulse, when the Suckers chest is enlarged; yet in our case that compression or protrusion will not reach the surface of the water in the shorter legg, because it is there fenc'd from the action of the external Air by the sides of the Glass, and the Hermetical Seal at the top. And yet, if one suck'd strongly at the open orifice in the

longer legg, the water in the shorter would be deprest; and that in the longer ascended at one suck about an Inch and half: Of which the reason is clear in our *Hypothesis*. For, the Spring of the included Air, together with the weight of the water in the shorter legg, and the pressure of the Atmospherical Air, assisted by the weight of the liquor in the longer legg, counter-ballanced one another before the Suction began: But, when afterwards upon Suction the Air in the longer legg came to be dilated and thereby weaken'd, 'twas render'd unable to resist the undiminish'd pressure of the Air included in the shorter legg, which consequently expanding it self by vertue of its Elasticity, deprest the contiguous water, and made it proportionably rise in the opposite legg, 'till by the expansion its Spring being more and more weaken'd, it arrived at an equallency with the gravitation or pressure of the Atmosphere. Which last clause contains the Reason, why, when  
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the person that suckt had rais'd the water in the longer legg *lesſ than* three Inches higher by repeated endeavours to suck , and that without once suffering the water to fall back again , he was not able to elevate the water in the longer , *so much as* three Inches above its first station. And if in the shorter legg there was but an Inch and a quarter of space left for the Air unfill'd by the water , by divers skilfully reiterated acts of Suction he could not raise the liquor in the longer legg above two Inches ; becaule by that time the Air included in the shorter legg had , by expanding it self further and further , proportionably weaken'd its Spring , 'till at length it became as rarified , as was the Air in the cavity of the longer legg , and consequently was able to thrust away the water with no more force than the Air in the long legg was able to resist. And by the recited tryal it appear'd , that the rarefaction usually made of Air by Suction is not near so great , as one would

C 3 expect,

expect, probably because by the dilatation of the Lungs the Air, being still shut up, is but moderately rarified, and the Air in the longer legg can by them be brought to no greater degree of rarity, than that of the Air within the Chest. For, whereas the included Air in our Instrument was not expanded, by my estimate, at one suck to above the double of its former dimensions, and by divers successive sucks was expanded but from one Inch and an half to less than four Inches and an half, if the Suction could have been conveniently made with a great and stanch Syringe, the rarefaction of the Air would probably have been far greater; since in our Pneumatick Engin Air may, without heat, and by a kind of Suction, be brought to possess many hundreds of times the space it took up before. From this rarefaction of the Air in both the leggs of our Instrument proceeds another *Phænomenon*, readily explicable by our *Hypothesis*. For if, when the water was impell'd

impell'd up as high as the Suction could raise it, the Instrument were taken from the Suckers mouth, the elevated water would with violence return to its wonted station. For, the Air, in both the leggs of the Instrument, having by the Suction lost much of the Spring, and so of its power of pressing; when once the orifice of the longer legg was left open, the Atmospheric Air came again to gravitate upon the water in that legg, and the Air, included in the other legg, having its Spring debilitated by the precedent expansion, was not able to hinder the external Air from violently repelling the elevated water, 'till the included Air was thrust into the space it possess'd before the Suction; in which space it had Density and Elasticity enough to resist the pressure, that the external Air exercis'd against it through the interpos'd water.

But our *Hypothesis* about the Cause of Suction would not need to be sollicitously prov'd to you by other ways,

C 4 if

if you had seen what I have sometimes been able to do in our Pneumatick Engin. For, there we found by tryals purposely devis'd, and carefully made, that a good Syringe being so conveyed into our Receiver, that the open orifice of the Pipe or lower part was kept under water, if the Engin were exhausted, though the handle of the Syringe were drawn up, the water would not follow it, which yet it would do if the external Air were let in again. The Reason of which is plain in our *Hypothesis*. For, the Air, that should have prest upon the surface of the stagnant water, having been pump't out, there was nothing to impell up the water into the deserted cavity of the Syringe, as there was when the Receiver was fill'd with Air.

CHAP.

## C H A P. IV.

But because such a conveniency as our Engin, and the *apparatus* necessary for such Tryals are not easily procurable, I shall endeavour to confirm our *Hypothesis* about Suction by subjoining some Experiments, that may be tryed without the help of that Engin, for the making out these three things :

I. That a Liquor may be rais'd by Suction, when the pressure of the Air, neither as it has Weight nor Elasticity, is the Cause of the Elevation.

II. That the weight of the Atmospherical Air is sufficient to raise up Liquors in Suction.

III. That in some cases Suction will not be made, as, according to the Hypothesis I dissent from, it should, although there be a dilatation of the Suckers Thorax, and no danger of a Vacuum though the Liquor should ascend.

And

And first, to shew, how much the rising of Liquors in Suction depends upon the weight or pressure of the impellent Body, and how little necessity there is, where that pressure is not wanting, that, in the place deserted by the Liquor that is suck'd, there should succeed Air or some other visible Body, as the *Peripatetic Schools* would have it; to shew this, *I say*, I thought on the following Experiments. We took a Glass-pipe fit to have the *Torriceillian* Experiment made with it, but a good deal longer than was necessary for that use: This Pipe being Hermetically seal'd at one end, the other end was so bent as to be reflected upwards, and make as it were the shorter legg of the Syphon as parallel as we could to the longer, so that the Tube now was shap'd like an inverted Syphon with leggs of a very unequal length. This Tube, notwithstanding its inconvenient figure, we made a shift, (for 'tis not easily done) to fill with Mercury, when 'twas in an inclin'd posture,

posture, and then erecting it, the Mercury subsided in the longer legg, as in the *Torriceilian Experiment*, and attain'd to between two foot and a quarter and two foot and an half above the surface of the Mercury in the shorter legg, which in this Instrument answers to the stagnant Mercury in an ordinary *Barometer*, from which to distinguish it I have elsewhere call'd this *Syphon*, furnish'd with Mercury, a *Travelling Baroscope*, because it may be safely carried from place to place. Out of the shorter legg of this Tube we warily took as much Mercury as was thought convenient for what we had further to do, and this we did by such a way as to hinder any Air from getting into the deserted cavity of the longer legg, by which means the Mercurial Cylinder, (estimated as I lately mention'd) retain'd the same height above the stagnant Mercury in the shorter: The upper and clos'd part of this Travelling Baroscope you will easily grant to have been free from Common Air,

Air, not only for other Reasons that have been given elsewhere, but particularly for this, that, if you gently incline the Instrument, the Quicksilver will ascend to the top of the Tube; which you know it could not do, if the place, formerly deserted by it, were possest by the Air, which by its Spring would hinder the ascension of the Mercury, (as is easie to be tryed.) The Instrument having been thus fitted, I caus'd one of the bystanders to suck at the shorter legg, whereupon (as I expected) there presently ensued an Ascension of four or five Inches of Mercury in that legg, and a proportionable Subsidence of the Mercury in the longer, and yet in this case the raising of the Mercury cannot be pretended to proceed from the pressure of the Air. For, the weight of the Atmosphere is fenc'd off by that, which closes the upper end of the longer Tube, and the Spring of the Air has here nothing to do, since, as we have lately shewn, the space deserted by the Mercury is  
not

not possest by the included Air, and the pulsion or condensation of the Air, suppos'd by divers modern Philosophers to be made by the dilatation of the Suckers Chest, and to press upon the surface of the Liquors that are to be suck'd up, this, *I say*, cannot here be pretended in regard the surface of the Liquor in the longer legg is every way fenc'd from the pressure of the ambient Air. So that it remains, that the Cause, which rais'd the Quicksilver in the shorter legg upon the newly recited Suction, was the weight of the collaterally superior Quicksilver in the longer legg, which, being (at the beginning of the Suction) equivalent to the weight of the Atmosphere, there is a plain reason, why the stagnant Mercury in the shorter legg should be rais'd some Inches by Suction; as Mercury stagnant in an open Vessel will be rais'd by the weight of the Atmosphere, when the Suction is made in the open Air. For, in both cases there is a Pipe, that reaches to the stagnant Mercury,

Mercury, and a competent weight to impel it into that Pipe; when the Air in the cavity of the Pipe has its Spring weaken'd by the dilatation that accompanied Suction.

The *Second* point formerly propos'd, which is, *That the weight of the Air is sufficient to raise Liquors in Suction*; may not be ill prov'd by Arguments legitimately drawn from the *Torriceilian Experiment* it self, and much more clearly by the first and fifteenth of our *Continued Physico-Mechanical Experiments*. And therefore I shall only here take notice of a Phænomenon, that may be exhibited by the *Travelling Baroscope*, which, though it be much inferiour to the Experiments newly referr'd to, may be of some use on the present occasion.

Having then provided an Instrument like the *Travelling Baroscope*, mention'd under the former Head, but whose leggs were not so unequally long, and having in it made the *Torriceilian Experiment* after the manner lately describ'd; we order'd the matter

ter so, that there remain'd in the shorter legg the length of divers Inches unfill'd with stagnant Mercury. Then I caus'd one, vers'd in what he was to do, so to raise the Quicksilver by Suction to the open orifice of the shorter legg, that, the orifice being seasonably and dexterously closed, the Mercury continued to fill that legg, as long as we thought fit; and then having put a mark to the surface of the Mercury in the longer legg, we unstopp'd the orifice of the shorter; whereupon the Mercury, that before fill'd it, was *depress'd*, till the same Liquor in the longer legg was rais'd five Inches or more above the mark, and continu'd at that height. I said, that the Mercury that had been raised by Suction, was *depress'd*, rather than that it *subsided*, because its own weight could not here make it fall, since a Mercurial Cylinder of five Inches was far from being able to raise so tall a Cylinder of Mercury as made a counterpoise in the longer legg; and therefore the depression we speak of,

of, is to be referr'd to the gravitation of the Atmospheric Air upon the surface of the Mercury in the shorter legg : And I see no cause to doubt but that , if we could have procured an Instrument , into whose shorter legg a Mercurial Cylinder of many Inches higher could have been suck'd up , it would by this contrivance have appear'd , that the pressure of the Atmosphere would easily impel up a far taller Cylinder of Mercury than it did in our recited Experiment.

That this is no groundless conjecture may appear probable by the Experiment you will presently meet with. For if the gravity of an incumbent Pillar of the Atmosphere be able to compress a parcel of included Air as much as a Mercurial Cylinder , equivalent in weight to between thirty and five and thirty foot of water , is able to condense it , it cannot well be denied that the same Atmospheric Cylinder may be able by its weight to raise and counter-ballance

ballance eight or nine and twenty Inches of Quicksilver, or an equivalent pillar of water in Tubes, where the resistance of these two Liquors to be rais'd and sustain'd by the Air, depends only upon their own unassisted gravity.

To confirm our Doctrine of the Gravitation of the Atmosphere upon the surface of the Liquors expos'd to it, I will subjoin an Experiment, that I devis'd to shew, that the incumbent Air, in its natural or usual state, would compress other Air not rarified, but in the like natural state, as much as a Cylinder of eight or nine and twenty Inches of Mercury would condense or compress it.

In order to the making of this, I must put you in mind of what I have shewn elsewhere at large, and shall further confirm by one of the Experiments that follows the

*See the Authors Defence  
of the Doctrine touching  
the Spring and Weight of  
the Air, against Fr. Lu-  
nus, chap 5.*

next; namely, that about twenty nine or thirty Inches of Quicksilver

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will compress Air, that being in its natural or usual state (as to rarity and density) has been shut up in the shorter legg of our Travelling or Syphon-like Baroscope, into half the room that included Air possess'd before. This premis'd, I pass on to my Experiment, which was this:

We provided a *Travelling Baroscope*, wherein the Mercury in the longer legg was kept suspended by the counterpoise of the Air that gravitated on the surface of the Mercury in the shorter legg, which we had so order'd, that it reached not by about two Inches to the top of the shorter legg. Then making a mark at the place where the stagnant Mercury rested, 'twas manifest according to our *Hypothesis*, that the Air in the upper part of the shorter legg was in its natural state, or of the same degree of density with the outward Air, with which it freely communicated at the open orifice of the shorter legg; so that this stagnant Air was equally prest upon by the weight of

of the collaterally superior Cylinder of Mercury in the longer legg, and the equivalent weight of a directly incumbent pillar of the Atmosphere. Things being in this posture, the upper part of the shorter legg, which had been before purposely drawn out to an almost capillary smallness, was Hermetically seal'd, which, though the Instrument was kept erected, was so nimbly done by reason of the slenderness of the Pipe, that the included Air did not appear to be sensibly heated, though for greater caution we staid a while from proceeding, that, if any rarefaction had been produc'd in the Air, it might have time to lose it again. This done, we open'd the lower end of the longer legg, (which had been so order'd before, that we could easily do it, and without concussion of the Vessel,) by which means the Atmospherical Air, gaining access to the Mercury included in the longer legg, did, as I expected, by its gravitation upon it so compress the Air included in

the shorter legg, that , according to the estimate we made with the help of a Ruler, (for by reason of the conical figure of the upper part of the glas we could not take precise measures,) it was thrust into near half the room it took up before , and consequently , according to what I put you lately in mind of, endur'd a compression like that , which a Mercurial Cylinder of about twenty nine Inches would have given it.

This Experiment , as to the main of it , was for greater caution made the second time with much the like success ; and though it had been more easie to measure the Condensation of the Air , if , instead of drawing out and sealing up the shorter legg of the Instrument , we had contented our selves to close it some other way ; yet we rather chose to employ *Hermes's Seal* , lest , if any other course had been taken , it might be pretended , that some of the included Air , when it began to be comprest , might escape out at the not perfectly and strongly clos'd

clos'd orifice of the legg wherein 'twas imprison'd.

To make it yet further appear, how much the Ascension of Liquors by Suction depends upon Pressure, rather than upon Natures imaginary Abhorrence of a *Vacuum*, or the propagated Pulsion of the Air; I will subjoin an *Instance*, wherein that presum'd Abhorrence cannot be pretended. The Experiment was thus made:

A Glass-Syphon, like those lately describ'd, with one legg far longer than the other, was Hermetically seal'd at the shorter legg, and then by degrees there was put in, at the orifice of the longer legg, as much Quicksilver as by its weight suffic'd to compress the Air in the shorter legg into about half the room it posseſſ'd before; so that, according to the *Peripatetick* Doctrine, the Air must be in a state of preternatural Condensation, and that to a far greater degree, than (as I have tryed) 'tis usually brought to by Cold, intense enough to freeze water. Then measuring

D 3 the

the height of the Quicksilver in the longer Tube above the superficies of that in the shorter, we found it not exceed thirty Inches. Now, if Liquors did rise in Suction *ob fugam vacui*, there is no reason, why this Quicksilver in the longer part of the Syphon should not easily ascend upon Suction, at least 'till the Air in the shorter legg had regain'd its former Dimensions, since it cannot in this place be pretended, that, if the Mercury should ascend, there would be any danger of a *Vacuum* in the shorter legg of the Tube, in regard that the contiguous included Air is ready at hand to succeed as fast as the Mercury subsides in the shorter legg of the Syphon. Nor can it be pretended, that, to fill the place deserted by the Quicksilver, the included Air must suffer a preternatural rarefaction or discension; since 'tis plain in our case, that on the contrary, as long as the Air continues in the state whereto the weight of the Quicksilver has reduc'd it, it is kept in a violent state  
of

of compression; since in the shorter legg it was in its natural state, when the Mercury, poured into the longer legg, did by its weight thrust it into about half the room it took up before. And yet, having caus'd several persons, one of them vers'd in sucking, to suck divers times as strongly as they could, they were neither of them able, not so much as for a minute of an hour, to raise the Mercury in the longer legg, and make it subside in the shorter for more than about an Inch at most. And yet to shew you, that the Experiment was not favourably tryed for me, the height of the Mercurial Cylinder in the longer legg above the surface of that in the shorter legg was, when the Suction was tryed, an Inch or two shorter than thirty Inches, and the comprest Air in the shorter legg was so far from having been by the exsuction expanded beyond its natural and first dimensions, that it did not, when the contiguous Mercury stood as low as we could

make it subside, regain so much as one half of the space it had lost by the precedent Compression, and consequently was in a preternatural state of condensation, when it had been freed from that state as far as Suction would do it. Whence it seems evident, that 'twas not *ob fugam vacui*, that the Quicksilver did upon Suction ascend one Inch; for, upon the same score it ought to have ascended two, or perhaps more Inches, since there was no danger, that by such an ascension any *Vacuum* should be produc'd or left in the shorter legg of the Syphon; whereas, according to our *Hypothesis*, a clear cause of the Phænomenon is assignable. For, before the Suction was begun, there was an *Æquilibrium* or equipollency between the weight of the superiour Quicksilver in the longer legg, and a Spring of the comprest Air includ'd in the shorter legg: But when the Experimentor began to suck, his Chest being widen'd, part of the Air includ'd in the upper part of the longer

longer legg pass'd into it, and that which remain'd had by that expansion its pressure so weaken'd, that the Air in the shorter legg, finding no longer the former resistance, was able by its own Spring to expand it self, and consequently to depress the contiguous Mercury in the same shorter legg, and raise it as much in the longer.

But here a *Hydrostatician*, that heedfully marks this Experiment, may discern a difficulty, that may perhaps somewhat perplex him, and seems to overthrow our Explication of the Phænomenon. For he may object, that if the comprest Air in the shorter legg had a Spring equipollent to the weight of the Mercury in the longer legg, it appears not, why the Mercury should not be suckt up in this Instrument, as well as in the free Air; since, according to me, the pressure of the included Air upon the subjacent Mercury must be equivalent to the weight of the Atmosphere, and yet experience shews, that the weight

weight of the Atmosphere will, upon Suction, raise Quicksilver to the height of several Inches.

To clear this difficulty, and shew, that, though it be considerable, 'tis not at all insuperable, be pleased to consider with me, that I make indeed the *Spring* of the comprest Air to be equipollent to the *Weight* of the compressing Mercury, and I have a manifest reason to do it; because, if the Spring of the Air were not equipollent to that Weight, the Mercury must necessarily compress the Air farther, which 'tis granted *de facto* not to do. But then I consider, that in our case there ought to be a great deal of difference between the operation of the *spring* of the included *Air* and the *Weight* of the *Atmosphere*, after Suction has been once begun. For, the Weight of the Atmosphere, that impels up Mercury and other Liquors, when the Suction is made in the open Air, continues still the same, but the force or pressure of the included Air is equal to the counterpressure of

of the Mercury no longer than the first moment of the Suction; after which, the force of the imprison'd Air still decreases more and more, since this comprest Air, being further and further expanded, must needs have its Spring proportionably weaken'd; so that it need be no wonder, that the Mercury was not suckt up any more than we have related; for there was nothing to make it ascend to a greater height, than that, at which the debilitated Spring of the (included but) expanded Air was brought to an equipollency with the undiminish'd and indeed somewhat increas'd weight of the Mercurial Cylinder in the longer legg, and the pressure of the Aerial Cylinder in the same legg, lessen'd by the action of him that fuck'd. For whereas, when the orifice of this legg stood open, the Mercury was prest on by a Cylinder of the Atmospherical Air, equivalent to about thirty Inches of Quicksilver; by the mouth and action of him that fuck'd the Tube was freed

freed from the external Air, and by the dilatation of his *Thorax*, the neighbouring Air, that had a free passage through his wind-pipe to it, was proportionably expanded, and had its Spring and pressure weaken'd: By which means, the comprest Air in the shorter legg of the Syphon was inabled to impel up the Mercury, 'till the lately mention'd *Equilibrium* or equipollency was attain'd. And I must here take notice, that, as the Quicksilver was rais'd by Suction but a little way, so the Cylinder that was rais'd was a very long one; whereas, when Mercury is suck'd up in the free Air, it is seldom rais'd to half that length; though, as I noted before, the impellent cause, which is the weight of the Atmosphere, continued still the same, whereas in our Syphon, when the Mercury was suck'd up but an Inch, the comprest Air, possessing double the space it did before, had by this expansion already lost a very considerable part of its former Spring and Pressure.

I should here conclude this Discourse, but that I remember a *Phænomenon* of our Pneumatic Engin, which to divers Learned Men, especially *Aristotelians*, seem'd so much to argue, that Suction is made either by a *Fuga Vacui*, or some internal Principle, that divers years ago I thought fit to set down another account of it, and lately meeting with that account among other papers, I shall subjoin it just as I found it, by way of *Appendix* to the foregoing Tract.

Among the more familiar *Phæno-mena* of the *Machina Boyleiana*, (as they now call it,) none leaves so much scruple in the Minds of some sorts of Men, as this, That, when ones finger is laid close upon the orifice of the little Pipe, by which the Air is wont to pass from the Receiver into the exhausted Cylinder, the pulp of the finger is made to enter a good way into the cavity of the Pipe, which doth not happen without a considerable sensē of pain in the lower part

part of the finger. For most of those that are strangers to *Hydrostaticks*, especially if they be prepossess'd with the Opinions generally receiv'd both in the *Peripatetick* and other Schools, perswade themselves, that they feel the newly mention'd and painful protuberance of the pulp of the finger, to be effected not by pressure, as we would have it, but distinctly by Attraction.

To this we are wont to answer, That common Air being a Body not devoid of weight, the Phenomenon is clearly explicable by the pressure of it: For, when the finger is first laid upon the orifice of the Pipe, no pain nor swelling is produc'd, because the Air which is in the Pipe presses as well against that part of the finger which covereth the orifice, as the ambient Air doth against the other parts of the same finger. But when by pumping, the Air in the Pipe, or the most part of it, is made to pass out of the Pipe into the exhausted Cylinder, then there is nothing left in

in the Pipe, whose pressure can any thing near countervail the undiminish'd pressure of the external Air on the other parts of the finger; and consequently, that Air thrusts the most yielding and fleshy part of the finger, which is the pulp, into that place where its pressure is unresisted, that is, into the cavity of the Pipe, where this forcible intrusion causeth a pain in those tender parts of the finger.

To give some visible Illustration of what we have been saying, as well as for other purposes, I thought on the following Experiment.

We took a Glass-pipe of a convenient length, and open at both ends, whose cavity was near about an Inch in *Diameter*, (such a determinate breadth being convenient, though not necessary:) To one of the ends of this Pipe we caused to be firmly tyed on a piece of very fine Bladder, that had been ruffled and oyl'd, to make it both ~~ver~~ limber and unapt to admit water; and care was taken, that

the

the piece of Bladder tyed on should be large enough, not only to cover the orifice, but to hang loose somewhat beneath it.

This done, we put the cover'd end of the Pipe into a Glass-body (or Cucurbit) purposely made more than ordinarily tall, and the Pipe being held in such manner, as that the end of it reach'd almost, but not quite, to the bottom of the Glass-body, we caused water to be poured both into this Vessel and into the Pipe (at its upper orifice, which was left open) that the water might ascend equally enough, both without and within the Pipe. And when the Glass-body was full of water, and the same liquor was level to it, or a little higher within the Pipe, the Bladder at the lower orifice was kept plump, because the water within the Pipe did by its weight press as forcibly downwards, as the external water in the large Glass endeavour'd to press it inwards and upwards.

All this being done; we caus'd part

part of the water in the Pipe to be taken out of it, (which may be done either by putting in and drawing out a piece of Spunge or of Linnen, or more expeditiously by sucking up part of the water with a smaller Pipe to be immediately after laid aside,) upon which removal of part of the internal water, that which remained in the Pipe being no longer able, by reason of its want of weight, to press against the inside of the Bladder near as forcibly as it did before, the external water, whose weight was not less'n'd, press'd the sides and bottom of the Bladder, whereto it was contiguous, into the cavity of the Pipe, and thrusted it up therein so strongly, that the distended Bladder made a kind of either Thimble or Hemisphere within the Pipe. So that here we have a protuberance, like that above-mentioned of the finger, effected by Pulsion, nor Attraction; and in a case where there can be no just pretence of having recourse to Natures Abhorrence of a

Vacuum, since, the upper orifice of the Pipe being left wide open, the Air may pass in and out without resistance.

The like swelling of the Bladder in the Pipe we could procure without taking out any of the internal liquor, by thrusting the Pipe deeper into the water; for then the external liquor, having by reason of its increase of depth a greater pressure on the outside of the Bladder, than the internal liquor had on the inside of it, the Bladder must yield to the stronger pressure, and consequently be impell'd up.

If the Bladder lying loose at the lower end of the Pipe, the upper end were carefully clos'd with ones thumb, that the upper Air might not get out until the Experimentor thought fit, and if the thus clos'd Pipe were thrust almost to the bottom of the water, the Bladder would not be protuberant inwards, as formerly, because the included Air by virtue of its Spring, resisted from within

within the pressure of the external water against the outside of the Bladder : But if the thumb, that stopp'd the Pipes upper orifice, were remov'd, the formerly compress'd Air having liberty to expand it self, and its elasticity being weaken'd thereby, the external water would with suddenness and noise enough, not to be unpleasant to the Spectators, drive up the Bladder into the cavity of the Pipe, and keep it there very protuberant.

To obviate an Objection, that I foresaw might be brought in by persons not well vers'd in *Hydrostaticks*, I caus'd the Pipe fore-mention'd, or such another, to be so bent near the lower end, as that the orifice of it stood quite on one side, and the parts of the Pipe made an angle as near to a right one as he that blew it could bring it to. This lower orifice being fitted with a Bladder, and the Pipe with its contained liquor being thrust under water after the former manner, the lateral pressure

of the water forc'd the Bladder into the short and horizontal legg, and made it protuberate there, as it had done when the Pipe was straight.

Lastly, that the Experiment might appear not to be confin'd to one liquor; instead of *water* we put into the unbent Pipe as much red *wine* (whose colour would make it conspicuous) as was requisit to keep the Bladder somewhat swelling outwards, when it was somewhat near the bottom of the water; and then 'twas manifest, that, according as we had foreseen, the superficies of the red liquor in the Pipe was a good deal higher than that of the external water, and if the depth of both liquors were proportionably lessen'd, the difference of height betwixt the two surfaces would indeed, as it ought to happen, decrease, but still the surface of the wine would be the higher of the two, because being lighter in *specie* than the common water, the *Aequilibrium* between the pressures of the two liquors upon the Bladder would

would not be maintain'd, unless a greater height of *wine* compensated its defect of specifick gravity. And if the Pipe was thrust deeper into the water, then the Bladder would be made protuberant inwards, as when the Pipe had water in it. By which it appears, that these *Phænomena*, without recourse to *attraction*, may be explicated barely by the Laws of the *Æquilibrium* of Liquors.

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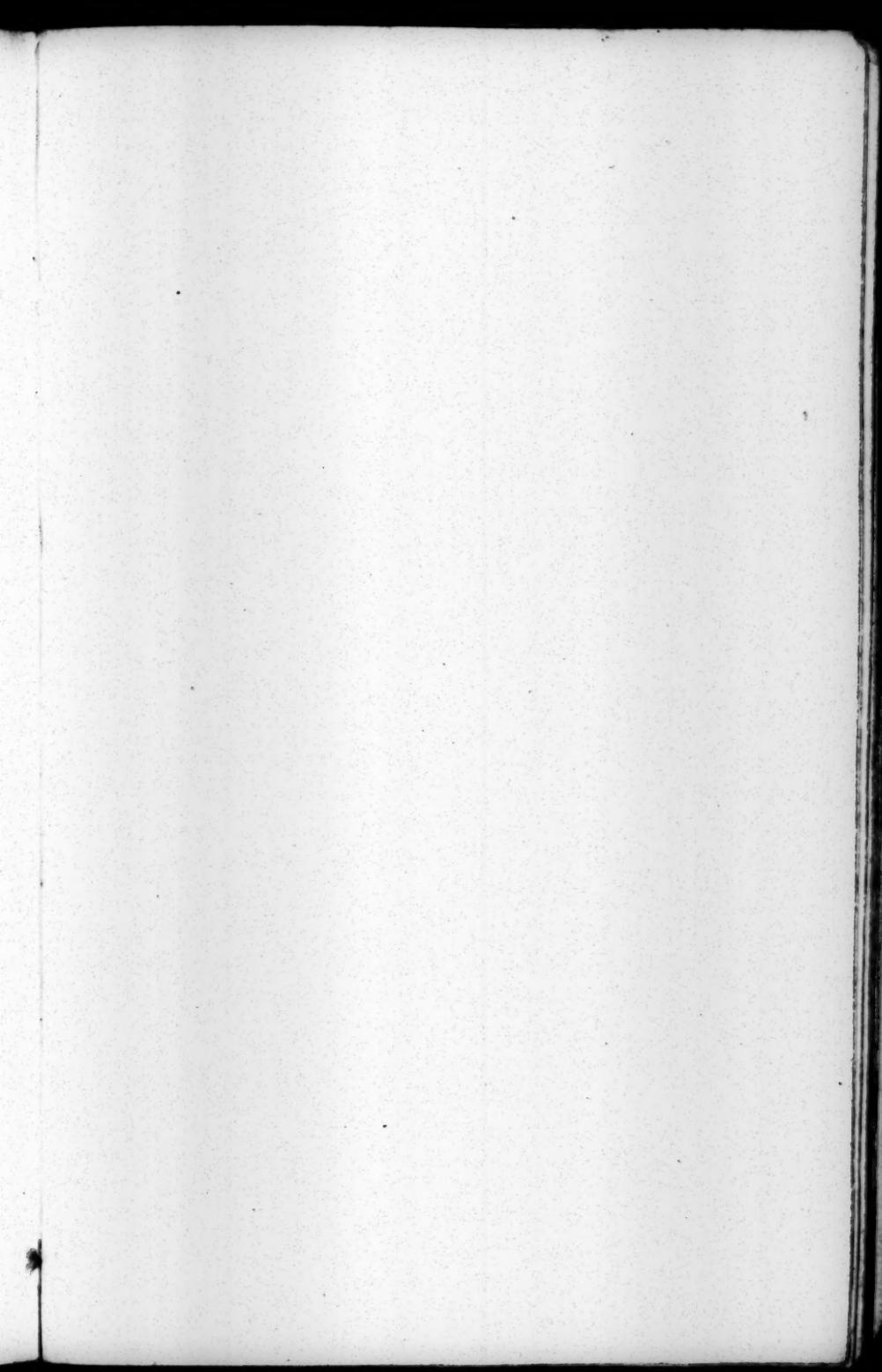
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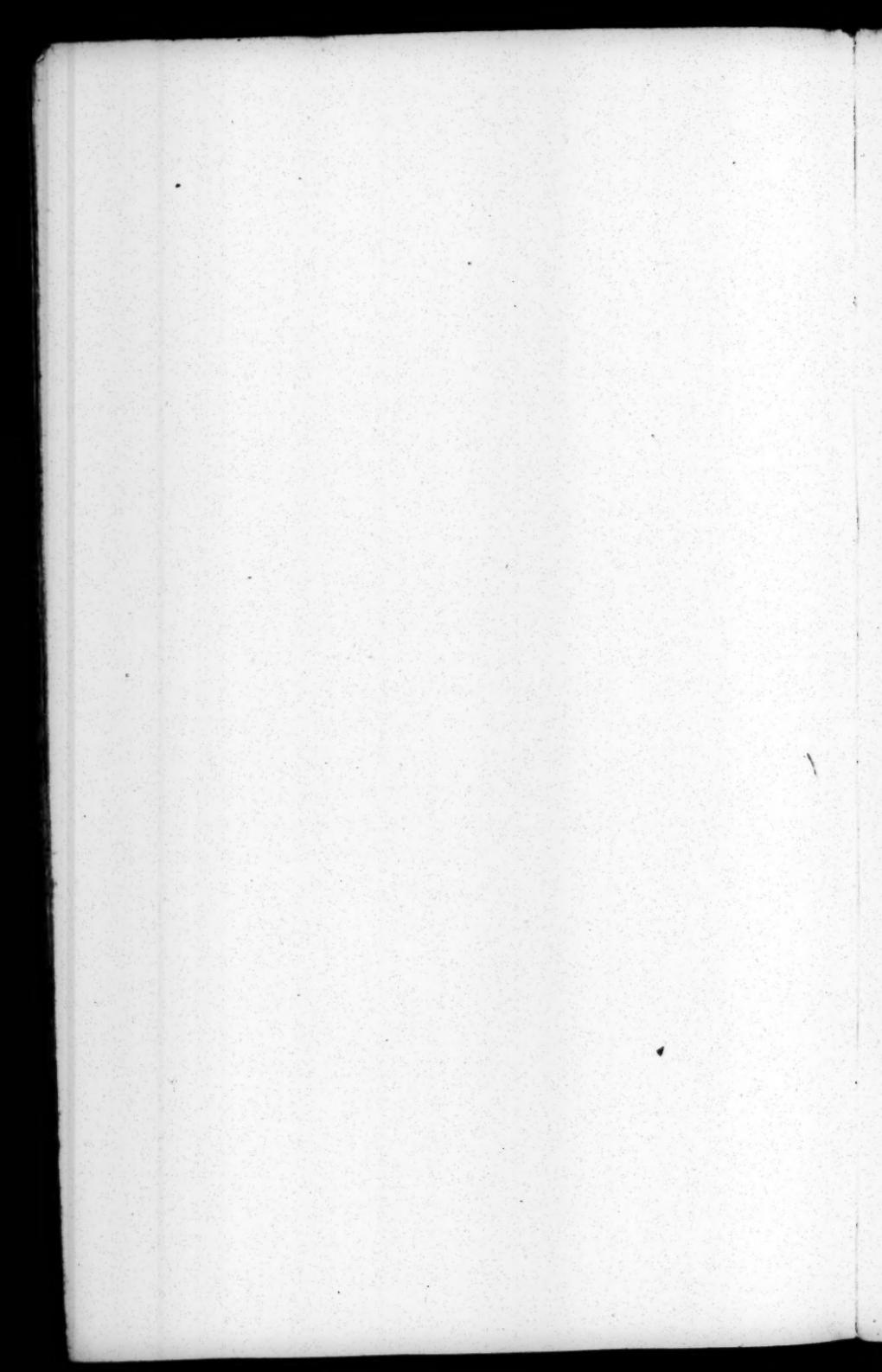
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18. **memoria** et **descensus**

et **adventus** **et** **resurrectionis** **christi** **hunc**  
tempore **comprobatur** **in** **to** **christi** **resurrec-**  
**tione** **quibus** **clavis** **christi** **et** **corona**  
**coronae** **regalis** **sumuntur** **ad** **hunc** **tempo-**  
**rem** **restituta** **est** **ad** **christum** **et** **ad** **restituta**  
**ad** **christum** **et** **ad** **restituta** **ad** **christum**  
**ad** **christum** **et** **ad** **restituta** **ad** **christum**  
**ad** **christum** **et** **ad** **restituta** **ad** **christum**







NEW  
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OF  
**BODIES**  
IN  
**VACUO BOYLIANO.**

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By the Honourable  
**ROBERT BOYLE,**  
Fellow of the Royal Society.

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## PREFACE.

**M**y willingness to make the bulk of the Papers about the Hidden Qualities of the Air less inconsiderable, by things that were of affinity to the Subject, inducing me to tumble over some of my Adversaria, I met among them with divers loose Notes, or short Memorials of some Experiments I made several years ago (and some of a fresher date) about the Preservation of Bodies by excluding the Air, wherefore I was easily persuaded to subjoin these to the Additional

## P R E F A C E.

ditional Experiments last recited.  
For it seems not yet clear, by what  
manifest Quality the Exclusion of  
the Air should so much contribute  
to keep from putrefaction variety  
of Bodies, that are usually found  
very much disposed to it. And  
therefore 'till the Cause of this  
Preservation be further penetrated,  
it may not be altogether impertinent  
to mention some Experiments rela-  
ting to it. And though these be  
only such as come now to hand, and  
were most of them set down rather  
as Notes than Relations, yet being  
faithfully register'd, and most of  
them having been made in Vacuo  
Boyliano (as they call it) they  
will probablly be New, and so per-  
haps not altogether useless to Na-  
turalists, who may vary them, and  
requite me for them, by trying the  
same

## P R E F A C E.

*same Experiments, I made by the Removal of the Air by the bare Exclusion of adventitious Air. For sometimes through hast I did not, and sometimes for want of convenience I could not, try, whether the same Phænomena would appear, if the same Bodies were shut up with Air in them, provided they were diligently kept from all commerce with the Air without them.*

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N E W





NEW  
**EXPERIMENTS**  
*ABOUT THE*  
**PRESERVATION OF BODIES**  
 IN  
*VACUO BOTLIANO.*

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*EXPER. I.*

**A** Piece of roasted Rabbet, being exactly clos'd up in an exhausted Receiver the Sixth of November, was two months and some few days after taken out without appearing to be corrupted, or sensibly alter'd in Colour, Tast, or Smell.

## EXPER. II.

A small Glass-Receiver, being half fill'd with pieces of White-bread, (part Crust and part Crumb) was exhausted, and secur'd the eleventh of *March*: The Receiver being open'd the first of *April*, part of the Bread was shaken out, and appear'd not to have been considerably, if at all sensibly, impair'd in that time, save that the outside of some pieces of Crumb seem'd to be a little, and but a little, less soft and white than before. There appear'd no drops or the least Dew on the inside of the Glass. The remaining Bread was again secur'd soon after.

The eighteenth of *April*, the Bread was taken out again, and tasted much as it did the last time, the Crust being also soft, and no drops of water appearing on the inside of the Glass.

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## EXPER. III.

This day (being the ninth of *March*) I open'd a small exhausted and secur'd Receiver, wherein, about the ninth of *December*, that is, about three months ago, we had included some *Milk*: Upon opening an access to the Air, we found the Milk well colour'd, and turn'd partly into a kind of Whey, and partly into a kind of soft Curd. The taſt was not offensive, only a little ſowriſh like Whey, and the ſmell was not at all ſtinking, but ſomewhat like that of ſowriſh Milk.

## EXPER. IV.

The *Violet-leaves*, that were put up, and freed and secur'd from Air the fifth of *March*, being this day open'd, (*April* the seventh) appear'd not to have chang'd their ſhape, or colour, or coniſtence: For, as for their odour, it could not be well judg'd of, because he that included them had, for

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#### 4 Experiments about the

his own ease, contrary to my express direction, crush'd many of them together in thrusting them down; and by such a violation of their Texture, it's natural for Violets to lose their fragrancy, and acquire an Earthy smell.

#### EXPER. V.

Having carefully placed some *Violets* in an exhausted Receiver, of a convenient size and bigness, and secur'd it from immediate commerce with the external Air; the Seventh month after we look'd upon them again, and found they were not putrified or resolved into any mucilaginous substance, but kept their shape intire, some of them retaining their colour, but more of them having so lost it, as to look like white Violets.

#### EXPER. VI.

November the fifth, we conveyed into a conveniently shap'd Receiver

## Preservation of Bodies.

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ver some ounces of *Sheeps-blood*, taken from an Animal that had been kill'd that afternoon. And after the exhaustion of the Air, during which, store of bubbles were generated in the Liquor that made it swell notably; the included Blood was kept in a place, (whose warmth we judg'd equal to that of a digestive Furnace) for twenty days; for one or two of the first of which, the Blood seem'd to continue fluid, and of a florid colour, which afterwards degenerated into one that tended more to blacknes. On the twenty fifth of *November* we came to let in the external, and found it to rush into the Receiver, and the Glass containing the Blood being held in a lightsom place, the most part of the bottom of it seem'd to be thinly overlaid with a coagulated substance of a higher colour than that which swam above it, which yet, though it appeared dark and almost blackish in the Glass whilst it was look'd on in the bulk, yet, if it was shook, those

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parts

parts of it that fell down along the inside of the Glass, appear'd of a deep but fair colour. But whilst the Blood continued in the Glass, it was suppos'd not to stink, since, even when it was poured out, though its smell seem'd to me (whose Organs of Smelling are tender) to have I know not what that was offensive, yet to others it seem'd to smell but as the Blood of a newly kill'd Dog.

## EXPER. VII.

Some *Cream* being put up and secur'd the seventeenth of *March* in an exhausted Receiver, did this day appear to be more thick and almost Butter-like at the top (whose *superficies* seem'd rugged) than otherwhere, and afterwards by being well shaken together in the not inconveniently shap'd Glass, was easily enough reduc'd to Butter, whose Butter-milk, by the judgment of those who were more us'd to deal in it than I, appear'd not differing from ordinary Butter-

## ¶ Preservation of Bodies.

Butter-milk. And I found it had, like that, a grateful sowness. The Butter was judg'd to be a little sower than ordinary, but was not, as they speak, *made*.

[In the Entry of this Experiment, Blanks were left for the years; but the Tenour of the words, and Design of the Experiment, and other Circumstances, assure me, that the Cream continued a year in the vessel.]

### EXPER. VIII.

February the eighteenth we look'd again upon three Vials, that had been exhausted and secur'd the fifteenth of September last, the one of these had in it some slices of *roasted Beef*, and the other some shivers of *white Bread*, and the last some thin pieces of *Cheese*; all which seem'd to be free from putrefaction, and look'd much as they did when they were first put up: Wherefore we thought not fit to let the Air into the Receiver, but left them as they were to lengthen the design'd Trial.

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## EXPER. IX.

February the eighteenth, there was a fourth Vial, wherein about six months before, *viz.* August the twelfth, had been inclos'd and secur'd some *July-flowers* and a *Rose*; and yet these being kept in the same place with the rest, though they seemed a little moist, retained their shape and colour, especially the *Rose*, which look'd fresh enough to seem to have been gather'd but lately.

*N. B.* That we observed not in any of these four Receivers any great drops, or so much as Dew in the upper parts, *viz.* those that were situated above the included matter.

## EXPER. X.

June the fourth we left some *Strawberries* in an exhausted Receiver, and coming to look upon them after the beginning of *November*, we found them to be discolour'd, but not alter'd  
in

## Preservation of Bodies. 9

in shape, nor affording any sign of Corruption by being at all mouldy: Wherefore we thought fit to leave them still in the Receiver for further Trial.

### EXPER. XI.

May the second, 1669, a piece of roasted Beef, secur'd September the fifteenth, appear'd to be not at all alter'd: As did likewise a piece of Cheese secur'd in another Receiver; and some pieces of a French Rose the same day (September the fifteenth) secur'd in a third.

N. B. The Flowers seal'd up August the twelfth, 1668, being this day look'd upon, appear'd fresh, and consequently did so after having been kept eight months and an half.

### EXPER. XII.

There was taken Beer of eight shillings a Barrel, of a year old, near a Pint of which, June the seventeenth, was

10 Experiments about the

was put into a conveniently shap'd Glass, and it was afterwards exhausted and secur'd from the Air; the most part of the month of *August* prov'd extraordinarily hot. Towards the latter end there was at several times great Thunder, which made the Beer in our Cellar, and in most of those of the Neighbourhood, turn soure. The first of *September*, the Beer was open'd, but did not seem to have degenerated into any soureness.

EXPER. XIII.

Being desirous to try, whether the *Thunder* would have such effect upon *Ale* exactly stopp'd in Glass-vessels, as it often has on that Liquor in the ordinary wooden Casks; I caus'd some Ale moderately strong to be put into a conveniently shap'd Receiver, and having exhausted the Air and secur'd a Glass-vessel, 'twas put into a quiet, but not cool, place: Last week, which was about six weeks after the Liquor had been inclos'd, there

there happening some very loud Thunder, and our Beer, though the Cask was kept in a good Cellar, being generally noted to have been turn'd soure after this Thunder; I staid yet a day or two longer, that the operation upon our included Liquor might be the more certain and manifest; and then permitting an access to the outward Air, we took out the Ale, and found it to be good drink, and not at all soured.

Compare this with the Wish made in the *Essay of the Great Efficacy of Effluviums*, chap. 5. pag. 28. that such an Experiment should be tried.

## EXPER. XIV.

September the twenty first, 1670, some Blackberries, included in an exhausted Receiver, were open'd June the twentieth, 1673, and were found free from all mouldiness and ill sent, only there was found some Liquor that was soure, which being taken out the Berries were secur'd again.

[At

[At the same time was another parcel of the same Berries exactly clos'd up in a Receiver, whence the Air was not pump'd, to try what difference in the Event would appear by this variation. But, coming in October the eleventh, 1673, to look upon the Glass, we found it crack'd, and the Fruit all cover'd over with a thick mould. Nor was this the only Vessel wherein Trials, made to preserve Fruits, without any exhaustion of the Air, miscarried.]

October the eleventh, 1674, the same Berries, being look'd upon, appear'd to have their colour alter'd, and much less black than before, but did not appear putrefied by either loss of shape, or by any stinking smell, nor was the least mouldiness observed to be on them, though they had been kept in the same Receiver above four year.

That *Fructus Horarii*, especially so tender and juicy ones, should without any additament be preserved from putrefaction so many times longer than otherwise they would have last'd,

lasted, as 'tis more than would be expected, so it may give hopes; that both odd and useful things of this kind may be this way performed.

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**POST.**

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On Saturday last there was a  
Meeting of the Committee of Supply  
and the following Goods and  

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Services were Committed →  
to the Committee of Supply and to  
the Committee of Correspondence  
and the Committee of Safety.

## POSTSCRIPT.

The foregoing Experiments, as the Memorials themselves declare, were all of them made *in Vacuo Boyliano*, nor did I intend to set down any other: But meeting among those Memorials with a short account of a couple of Trials made without the help of our Pneumatic Engine, I was induc'd to annex them, because many may make the like, that will not be able to make such as have been hitherto recited. And these two requiring no peculiarly shap'd Vessels, 'tis thought, it may prove of some Oeconomical as well as Physical use, if it be shewn by experience, that Liquors Hermetically-seal'd the ordinary way in common Bolt-heads may be kept from souring very much beyond their usual time of lasting.

June

June the fourteenth we put a convenient quantity of good Ale into a Bolt-head ; and seal'd it up Hermetically ; the next year, on the fifth of July, we broke off the Seal, and found the Liquor very good and without any sensible sowreness. The next day it was seal'd up again and set by for thirteen months, at which time the neck of the Glass being broken, the Ale was found pretty sowre, and therefore the Trial was prosecuted no farther : So that, though this Liquor would not by this way of Preservation be kept from sowring so long as the Wine, to be mention'd in the following Experiment, yet even a small quantity of it was preserved good at the least above a year, which is very much longer than Ale is wont to keep from sowring.

June the fourteenth, 1670, in a large Bolt-head was Hermetically seal'd up about a Pint, by gues, of French Claret-wine, which, when we

came to look upon, July the fifth, 1673; appear'd very clear and high colour'd, and had deposited store of foam at the bottom of the Glass, but susten'd no Tartar that we could perceive to the sides. Upon the breaking of the seal'd end of the Glass, the By-standers thought, that there was an eruption of included Air or steams, and above the surface of the Wine, there appear'd to a pretty height, a certain white smoak almost like a misty, and then gradually vanished. The Wine continued well-tasted, and was a little rough upon the tongue, but not at all sour, or noisy-violent to the Bolt-head was seal'd up again July the sixteenth 1673, and so left by 'till August the fifth 1673, at which time it was open'd again, and then the Wine did still last very well.

June the twenty-sixth 1673, the Bolt-head with the same Claret-wine was open'd, and was found very good, and was seal'd up again.

October the eleventh 1674, the same Claret-wine was open'd again, and

and appear'd of a good colour, not sowre, but seem'd somewhat less spirituous than other good Claret-wine, perhaps because of the Cold weather.

This, and the foregoing Trial about the Preservation of Ale, were made in Mr. Oldenburg's House and Presence.

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F I N I S.

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